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ART. XXX.—*Biography of Berzelius;* by Prof. H. ROSE, of Berlin.

(Continued from p. 186.)

SOMETIME after the appearance of the mineral system, Berzelius published his work "*Ueber die Anwendung des Löthrohrs in der Chemie und Mineralogie.*" In Fahlun, under the guidance of his older friend Gahn, a pupil of Torbern Bergmann, he had acquired uncommon dexterity in the use of the blowpipe; and he had thus enriched this special part of chemistry with a number of original investigations, and brought it to a high degree of perfection. In the above-mentioned work he makes known everything connected with the subject, both what he had learned from Gahn and what he had himself discovered.

Rarely has a work been welcomed by chemists like this; indeed, so immediate a recognition of the practical value of a work is seldom possible. It was at once translated into most European languages; and in some, especially the German, it passed through several editions. Everywhere it met with merited appreciation; and Mr. Children alone, the editor of the English edition, allowed himself to add remarks as superfluous as they were ill-natured.

Besides the behavior before the blowpipe of the most important chemical compounds, all metallic oxyds, acids, and their salts, sulphurets, &c., Berzelius described the reaction of all minerals which he could obtain, and which were so much the more readily placed at his disposal, as he required only very small quantities for these experiments. He entered upon this investi-

gation with untiring industry, and was thus able to furnish even those mineralogists who but unwillingly admitted the influence of chemistry upon mineralogy, with an extremely welcome gift, since, by simple blowpipe experiments, it was made possible to distinguish minerals with ease and certainty, especially siliceous compounds, which were with difficulty, or only ambiguously, recognizable by means of their external characters.

This work bore so manifestly the stamp of perfection, even on its first appearance, that, with the exception of Plattner, in Freiberg, no one has contributed any essential additions or improvements to blowpipe investigations; and it is quite as indispensable to the chemist and mineralogist at the present day as it was thirty years ago.

About this time, Berzelius discovered selenium, and was engaged upon his admirable investigation of this element. Never was there an examination so accurate and thoroughly exhaustive, of an interesting and hitherto unknown element, comprising all its characters and remarkable combinations, so that, if we except the discovery of selenic acid by Mitscherlich, which escaped Berzelius, nothing essentially new was added to our knowledge of this element during the next thirty years. Our astonishment at this will be the greater when it is recollected that all these investigations were carried on with a very small quantity, only about an ounce of selenium, of which a part was lost through the carelessness of a servant.

This paper upon selenium can be compared only with that by Gay-Lussac upon iodine, which appeared several years before, and has yielded, in so many respects, such valuable results. It must, nevertheless, be remarked, that Gay-Lussac was not the discoverer of iodine, and did not undertake the investigation until after the first chemist of that time, Davy, had almost established the true nature of iodine; and that he had large quantities of materials at his disposal.

Almost at the same time that Berzelius was engaged in the examination of the compounds of selenium, Arfvedson was engaged in his laboratory with the analysis of some Swedish minerals; and under the guidance of Berzelius, succeeded in discovering lithium, which, as it came so unexpectedly, justly created great interest.

The following larger papers of Berzelius form, as it were, a series of monographs upon separate and important branches of chemistry, which were at that time still obscure. It was natural, when he commenced the demonstration of the law of definite proportions by means of a succession of laborious investigations, that he should throw aside much, in order to sketch the groundwork of his system. The investigations which he now undertook, were all instituted in accordance with a matured plan, and he had long meditated upon them before actually undertaking them.

The first of these researches was upon the ferruginous cyanogen compounds. Gay-Lussac, in his very important paper upon cyanogen, had neglected to study these compounds. After him, several chemists had occupied themselves with their examination, but all obtained very different results, the greater number, however, assuming that the iron in the so-called ferro-prussic acid salts was an essential constituent of the acid, which was combined in the salts with an oxydized body.

Berzelius, however, shewed that these salts contained neither prussic acid nor oxydized bases, but that they consisted of cyanid of iron combined with the cyanid of an alkaline metal, and consequently were double cyanids. He also extended his investigations to the salts of the so-called sulpho-cyanic acid, and shewed that they consist of a metal, sulphur, and cyanogen, the latter two united to form a radical (which he subsequently called Rhodan); and that in them likewise there was neither prussic acid nor oxydized bases.

These investigations, which fully confirmed the views of Gay-Lussac regarding cyanogen, were, however, of still greater importance to Berzelius in another respect. After Davy had been induced, by his researches in 1810, to consider that it was simpler and more correct to look upon chlorine as elementary, and not, as he had formerly done, as a compound of oxygen with a radical that had not been isolated; most chemists concurred with him in this view. Gay-Lussac and Thénard, who, even before Davy, considered a similar view possibly correct, although not exactly more probable than the old one, after the discovery of iodine, openly declared themselves, with Vauquelin and all the other French chemists, in favor of the new doctrine; and the famous paper of Gay-Lussac upon iodine, which appeared in 1813, is written in this spirit.

Berzelius alone, who from the first had disputed the hypothesis of Davy, continued to defend the old doctrine, even after the discovery of iodine. He did this especially in a paper which first appeared in Gilbert's Annalen for 1815. He there endeavored, with a profound sagacity which must be highly admired by every one, on reading the paper even after the lapse of so long a time, to prove the truth of the doctrine of the compound nature of chlorine. He directed attention to the remarkable phenomenon that the constituents of chlorid of nitrogen, which are united only by a very feeble affinity, separate with such an energetic evolution of heat as is never observed except in chemical combinations. But above all he pointed out the analogy which existed between muriates, which, according to the new theory, in the anhydrous state contain no oxygen, and the sulphates, phosphates, and other salts, which are indisputably compounds of oxygen acids with oxygen bases, and in which the presence of oxygen may be readily detected.

The great authority of Berzelius, and the soundness with which he carried out his refutation of all the evidence brought forward in favor of the new theory, were the reasons why many chemists, especially in Germany, did not adopt Davy's view of the nature of chlorine.

The immediate cause that led Berzelius to undertake the investigation of the cyanids of iron was evident, viz., he expected to find in them a more complex radical (united with oxygen forming an acid) associated with an oxygen base, and similar to that which he assumed to exist in muriates. It cannot be disputed that to some extent he doubted the accuracy of Gay-Lussac's experiments on cyanogen. Then, as the salts of the ferrocyanic radicals resemble so closely in their characters the ordinary oxygenous salts, and especially as several metallic cyanids, such as cyanid of mercury or silver, correspond so completely with the analogous chlorine compounds, he was of opinion that if he could by this investigation detect oxygen in the ferrocyanic compounds, it would be a strong proof of its presence in muriates likewise, and, consequently, evidence in favor of the old theory of the nature of chlorine.

However, the result of these investigations was the opposite of that which he expected, and thus the main argument against the new doctrine of the nature of chlorine fell to the ground. When other reasons for the greater probability of the new theory were from time to time discovered, Berzelius adopted it with the most amiable candor, and relinquished the old theory which he had so long and so ably defended.

One, among the reasons was, as I know, the following:— Immediately after Berzelius's investigations on the cyanids of iron, Leopold Gmelin obtained the interesting red double salt of cyanid of potassium and cyanid of iron, which is anhydrous and contains no oxygen. The red color of peroxyd of iron, which is more or less communicated to all its salts except the neutral ones, was to Berzelius an additional reason for regarding the red perchlorid of iron as an actual salt with an oxygenous base; and, as in the salt obtained by Gmelin, notwithstanding its red color, the iron was not in the state of oxyd, but directly combined with cyanogen, (one double atom of iron with three double atoms of cyanogen,) Berzelius saw that it was probable that the red color of iron compounds was not owing alone to the presence in them of peroxyd, but was also common to those in which one double atom of iron is combined with three double atoms of chlorine or cyanogen.

Another main inducement to adopt the new theory of the nature of chlorine, consisted in the results which he derived in favor of it from his subsequent comprehensive researches upon alkaline sulphurets. According to Berthollet's investigations, these

bodies were regarded as combinations of sulphur with alkalies until Vauquelin put forward the opinion, that when a fixed alkali was melted with sulphur, a part of the alkali was reduced to the metallic state, sulphuric acid was formed, and a mixture of alkaline sulphate and sulphuretted metal was obtained. This which Vauquelin was able to put forward only conjecturally, and could not demonstrate by convincing proofs, was immediately proved most satisfactorily by Berzelius through his successful reduction of sulphate of potash by means of hydrogen or the vapor of the sulphuret of carbon. He thus obtained sulphuret of potassium in which there could not be any oxygen. By treating anhydrous lime with sulphuretted hydrogen at a high temperature, Berzelius likewise obtained water and sulphuret of calcium. This experiment rendered it obvious that when liver of sulphur is obtained by melting together sulphur and carbonate of potash, the solution in water contains sulphuric acid, which is not, as Berthollet conjectured, first formed by the decomposition of water, but is a joint product with the liver of sulphur of the reduction of the alkali. Berzelius found, moreover, that the alkaline metals combine in several definite proportions with sulphur forming substances which are all soluble in water. Thus arose the question: What is contained in such a solution?—a question, the answer to which is especially important when regarded in connection with the solutions of metallic chlorids. Is this liquid a solution of the unaltered sulphuret in water, or is the alkaline metal oxydized, and, consequently, a compound of sulphuretted hydrogen with alkali formed, or a compound of sulphuretted hydrogen, sulphur, and alkali? Since, in the last case, it would be necessary to assume as many compounds of sulphur with hydrogen as there are compounds of sulphur with the alkaline metals, Berzelius decided in favor of the second view. Subsequent investigations of the solution of sulphur compounds of the metals of alkaline earths in water, have, in fact, shewn that a decomposition of water really does take place in this case, and that a compound of metallic sulphuret with sulphuretted hydrogen and alkaline oxyd is formed.

Berzelius regarded these investigations as proving that sulphur compounds exist which are very analogous to the muriates, and that there might likewise be bodies which, without containing an acid and an oxygenous base, possess, like the chlorids, all the peculiar characters of salts; and, consequently, if this were so, all that evidence against the new theory of chlorine failed, which he had derived from the perfect analogy of muriates with salts consisting of an oxygen acid and an oxygenous base.

With this investigation on alkaline sulphurets was connected the equally important one upon the sulphur salts, which, however, did not appear until several years afterwards.

In the former paper Berzelius had directed attention to the fact, that the sulphur compounds of alkaline and earthy metals combine with other metallic sulphurets in the same way as the oxyds of these metals combine with other oxyds. Double sulphurets are thus formed which admit of being compared with ordinary salts, inasmuch as one metallic sulphuret constitutes the electro-positive, that is, the basic part of the compound; the other, on the contrary, the electro-negative part, representing the acid. But here only the lowest sulphurets of the alkaline metals, that is, those corresponding as regards their composition with the basic oxyds of these radicals, will fill the place of basic sulphurets; the higher sulphurets behave, as it were, like peroxyds; they may sulphurize other metals, but do not combine with their sulphur compounds.

The different sulphur compounds of the electro-negative metals which Berzelius called sulphids, and whose composition is analogous to that of the metallic acids, combine with the electro-positive or basic sulphurets in such proportions, that if the sulphur were replaced by an equal number of atoms of oxygen, some one of the salts would be formed which the same radicals would yield in their oxydized state.

Of the sulphur compounds of the non-metallic elements, those of carbon and hydrogen alone combine with the basic sulphurets of the metals; the latter class of compounds,—those of sulphuretted hydrogen with alkaline sulphurets,—were already known under the name of hydrothio-alkalies, but their true composition was not recognized until now.

Berzelius regarded this extensive series of sulphur compounds appropriately as salts, and gave them the suitable name of sulphur salts, in order to distinguish them from the oxygen salts, or those which had been long known, and the so-called haloid salts, under which name Berzelius comprised the compounds of chlorine, bromine, iodine, fluorine, and cyanogen, as well as other compound radicals with metals.

This discovery of sulphur salts, is indisputably one of the most important extensions of chemistry. Berzelius entered upon their study with great industry, and the number of sulphur salts examined by him amounted to about 120, to many of which he certainly could give only a passing attention, although he analyzed many quantitatively.

Next to this followed his investigation of hydrofluoric acid, one of the most important which Berzelius executed, since it has thrown such an unexpected light upon several of the most interesting departments of chemistry.

Thénard and Gay-Lussac had indeed already prepared hydrofluoric acid in a pure state, and several of its compounds. But as they were at the same time occupied with a number of other im-

portant researches, they did not pursue this subject further, and especially did not study with sufficient accuracy the phenomena which presented themselves when potassium was heated in fluorid of silicium.

Berzelius, in the first instance, prepared the most important metallic fluorids; then he went on to the remarkable compounds which hydrofluoric acid forms with electro-negative fluorids, especially fluorid of silicium, and fluorid of boron, besides fluorid of titanium and others. It was through him that we first acquired a correct conception of the composition of hydrofluosilicic acid and the fluosilicates, as well as of the action of water upon fluorid of silicium. But the most productive part of this investigation was when Berzelius repeated the experiments of Gay-Lussac and Thénard, for the purpose of decomposing fluorid of silicium by means of potassium. He had just at this time learned from Wöhler how to prepare potassium by means of carbonate of potash and carbon according to Brunner's method, and thus provided himself with large quantities of this metal. On decomposing fluorid of silicium by potassium he obtained the same results as the French chemists: namely, the brown non-metallic substance which they regarded as a complex compound of fluosilicid of potassium and of fluorid of potassium with silica. Berzelius found that it was impure silicium, which, when washed with water, could be obtained free from all fluorine compounds. It then contained only an admixture of silica, which could be extracted by concentrated hydrofluoric acid, after having previously been slowly heated to redness. He moreover shewed that the silicium could be obtained in different states of density, and with different characters.

This unexpected result induced him to undertake similar investigations with fluorid of boron. We are indebted to him for a correct knowledge of the decomposition of fluorid of boron by water, and of the composition of fluoborids, as well as an easy method of preparing boron, by treating fluoborid of potassium with potassium. He likewise discovered at this time the gaseous chlorid of boron, and corrected the views of the composition of boracic acid by his own experiments and those of Arfvedson. He moreover prepared the compounds of fluorid of titanium with metallic fluorids, especially fluorid of potassium, from which body he shewed how metallic titanium was to be obtained by means of potassium. This is the only method by which titanium can be obtained in a pure state; for the experiments of Wöhler have proved that the substance found in the slags of iron furnaces, and formerly called metallic titanium, contains nitrogen and cyanogen. The compounds of fluorid of tantalum with metallic fluorids were also prepared, and he obtained metallic tantalum in the same way as titanium. He then reduced zirconium from the zirconio-fluorid

of potassium by means of potassium, studied the characters of zirconia, and finally turned his attention to the double compounds of fluorid of molybdenum and fluorid of tungsten with metallic fluorids, of which, however, he prepared only the compounds of fluorid of potassium with molybdate and tungstate of potash.

Berzelius had intended to pursue further these very interesting investigations of fluorine compounds. But when he found that a distinguished French chemist had also commenced the study of fluorine compounds, and had already named some newly discovered ones, he gave up this intention.

It must be remarked, that in these investigations Berzelius assumed, as he had previously done in the case of hydrochloric acid, that fluoric acid was an oxygen acid, and that it contained a radical, combined with two atoms of oxygen. But in the same year that he gave up the study of fluorine compounds, viz., in 1825, he observed in the first part of the third German edition of his "Lehrbuch," that it was more probable that fluoric acid, like hydrochloric acid, was a hydrogen acid; and he described all the fluorine compounds according to this view.

Together with these comprehensive researches, Berzelius published a number of less extensive ones. They all originated in his meeting with a number of doubtful statements while editing his "Lehrbuch," in reference to which he immediately instituted experiments in his laboratory, for the purpose of quickly deciding upon them. From among these I will here mention only the research upon chlorid of lime, which was formerly regarded, according to Gay-Lussac, as a compound of chlorine with lime, and the chlorids of potash and soda which were likewise regarded as similar in composition. Berzelius, on the contrary, directly after adopting the view of the elementary nature of chlorine, declared these bleaching compounds to be mixtures of metallic chlorids with salts, containing an oxyd of chlorine as an acid. He was of opinion, as he did not closely examine the subject, that the acid was chlorous acid, until the researches of Balard proved that it was hypochlorous acid.

Berzelius proved, that all the other explanations of the composition of the bleaching compounds were incorrect, by shewing that these contained an oxyd of chlorine. He dissolved in a solution of carbonate of potash, as much chlorid of potassium as it would take up, and passed chlorine through the liquid without saturating it. After a few minutes chlorid of potassium was precipitated, which contained no chlorate of potash, or scarcely any; the liquid had acquired the power of bleaching. When the liquid was separated from the precipitated chlorid of potassium, and perfectly saturated with chlorine, chlorate of potash was precipitated, containing scarcely any chlorid of potassium. Consequently, during the first action of the chlorine, chlorid of potassium must

have been formed from potash, the oxygen of which could have combined only with chlorine, giving rise to the production of the bleaching compound.

It had long been the wish of Berzelius to investigate the rare metals accompanying platinum, the knowledge of which had been left imperfect by the chemists who discovered them. He was enabled to carry this into execution, when, after the discovery of the large quantities of platinum in the Ural, he received through Herrn von Cancrin, a considerable quantity of native platinum, as well as native Osmium-Iridium. This circumstance led him into a very important investigation of the process for decomposing native platinum ores, by means of which the rare metals accompanying platinum were first properly made known. He studied the characters, determined the atomic weights of Rhodium, Palladium, Iridium, and Osmium, and prepared a number of their compounds. Owing to the great number of the oxyds and chlorids of these metals, and their great similarity to each other, this investigation was very difficult; and, as regarded osmium and osmic acid, a very unpleasant one. But although Berzelius himself declared that he had as it were given only the sketch of the history of these metals, still this research, like all that came from his hands, was an extremely accurate, and to a certain extent, perfect one.

The next investigation of Berzelius was in reference to a new and peculiar earth, Thoria, which he had discovered in a mineral from Brevig, in Norway. He had previously, on examining the minerals near Fahlun, found an earthy substance in very small quantity, which he regarded, although not with certainty, as a new earth, which he called Thoria; subsequently, however, he convinced himself that it was phosphate of Yttria. Since the newly discovered earth resembled, in some of its peculiar characters, alumina, he called this likewise Thoria; the mineral in which he had detected it, he named Thorite, and the metal which he obtained from its volatile chlorine compound, Thorium. Thoria belongs to a group of earths very similar in character to zirconia, several of which Svanberg, Bergemann, and Sjögren have recently discovered. At first Berzelius assumed that thoria contained only one atom of oxygen; the experiments, however, which he made for the purpose of determining the atomic weights of the metal and earth, are probably not quite decisive, and it is perhaps more likely that the earth is composed of two atoms of metal and three atoms of oxygen.

(To be continued)

ART. XXXI.—*On an Isothermal Oceanic Chart, illustrating the Geographical Distribution of Marine animals;* by JAMES D. DANA.

(Continued from p. 167.)

Remarks on the Several Temperature Regions.

THE form and varying breadth of the different regions, and the relations between the sea-temperatures of coasts in different latitudes, which they exhibit, are points demanding special remark. The conclusions are of much interest, although some changes in the chart will undoubtedly be required by future researches.

Atlantic Torrid Region, between 74° F. *north, and* 74° F. *south.*—The form of this region is triangular, with the vertex of the triangle to the east. Its least width is four degrees of latitude; its greatest width between the extreme latitudes, is forty-six and a half degrees. On the African coast it includes only a part of the coast of Guinea, and no portion is south of the equator. On the west, it embraces all the West India Islands and reefs (excepting the Little Bahama,) and the South American coast, from Yucatan to Bahia,—a fact that accounts for the wide distribution of marine species on the American side of the ocean.

Atlantic Subtorrid Regions, between 74° *and* 68° F.—The *North Subtorrid Region* of the Atlantic is about six degrees in its average width, which is equivalent to a degree of Fahrenheit to each degree in surface. It encloses within the same temperature limits, a part of the east coast of Florida, between 24° and 27½° north, and a part of the African coast, between the parallels of 9° and 14½° north, the two related coasts differing ten degrees in latitude. The Bermudas, in latitude 33°, and the Cape Verdes, in 15½°, fall within this region.

The *South Subtorrid Region* has the same average width as the northern.

Taking the whole Atlantic Torrid or Coral-reef zone together, its width on the east is about twenty-one degrees, while on the west, it extends between the parallels of 30° south and 34° north, a breadth of sixty-four degrees. As many species will thrive under the temperature of any part of the Torrid zone, the geographical range of such species in the Atlantic may be very large, even from Florida and the Bermudas on the north, to Rio Janeiro on the south, a range of which there are actual examples.

Atlantic Warm Temperate Regions, between 68° *and* 62° F.—The *northern* of these regions has a breadth of fourteen and a half degrees along the west of Africa, and about seven degrees along the United States, to the south of Cape Hatteras, off the Carolinas, Georgia, and northern Florida. These shores and the Canaries are therefore in one and the same temperature zone.

The *southern* of these regions averages five degrees in width. The eastern limit on the African coast is sixteen to eighteen degrees to the north of the western on the South American coast.

Atlantic Temperate Regions, between 62° *and* 56° F.—The *north* Temperate Region is but a narrow strip of water on the west, terminating at Cape Hatteras, and having no place on the coast of the United States. To the east it widens, and embraces the Azores, and the African coast along Morocco, together with the Straits of Gibraltar, and a large part of the Mediterranean. Madeira lies upon its southern limit. It is, therefore, natural, that the same species should occur at the Azores, Madeira, and on the African coast, and be excluded wholly from the Atlantic coast of Europe. This according to Prof. Forbes, is the fact with the *Littorina striata*, besides other species. The coasts of Portugal and the Azores are in different regions.

The *South* Temperate Region extends to Maldonado at the mouth of the La Plata, from near the parallel of 30°; along the African coast it reaches over more than twice the number of degrees of latitude, to within five degrees of Cape Town.

Atlantic Subtemperate Regions, between 56° *and* 50° F.—The *northern* of these regions, like the preceding, can not be distinguished on the coast of the United States, as the lines of 50° and 56° F. with 62° fall together at Cape Hatteras. On the eastern side of the Atlantic, it occupies the coast of Portugal to latitude 42° north, having a width of five degrees. It thus corresponds on this coast to the so-called Lusitanian Region.

The *southern* includes the mouth of the La Plata on one side, and on the other the coast near Cape Town, beyond which it extends to the Cape of Good Hope or rather to Cape Lagulhas.

Atlantic Cold Temperate Regions, between 50° *and* 44° F.—The coast from Cape Cod to Cape Hatteras belongs to the *Northern* Cold Temperate Region. Passing easterly, this region is but a narrow line of water for thiry degrees of longitude, after which it expands, and finally terminates between Western Ireland and latitude 42° on the Spanish coast. The British Channel, the Bay of Biscay, and probably Vigo Bay, Spain, are within the limits of this region.

The *southern* embraces the coast of South America, along by Rio Negro for about five degrees, and passes wholly to the south of Africa.

Atlantic Subfrigid Regions, between 44° *and* 35° F.—The coast of Massachusetts, north of Cape Cod, of Maine and Newfoundland, and all Northern Britain, the Orkneys, Shetlands, and Faroe Islands, pertain to the *northern* Subfrigid Region; while the *southern*, includes the Falklands, Southern Patagonia, and Fuegia.

Atlantic Frigid Regions, beyond 35° F.—Greenland, Iceland, and Norway are within the *northern* of these regions, and the South Shetlands, Sandwich Land, and South Georgia, within the *southern*.

Pacific Regions.—A comparison of the regions of the Atlantic and Pacific, and especially of the limits of those commencing at the South American coasts, brings out some singular facts.

The *Torrid* region of the Pacific, near the American coast, embraces only seventeen and a half or eighteen degrees of latitude, all but one of which are north of the equator; while that of the Atlantic covers a long range of coast, and reaches to 15° south. The south *Subtorrid* Region has a breadth of about three degrees on the Peruvian coast, reaching to 4° south, or probably to Cape Blanco, while that of the Atlantic extends to Rio Janeiro, in 24° south. The *Warm Temperate* Region, if at all found north of Cape Blanco, $4\frac{3}{4}$° S., has a breadth of less than a degree, while that of the Atlantic extends to Rio Grande, in 33° south. The next or *Temperate* Region has a longer range on the South American coast, extending to Copiapo, in $27\frac{1}{2}$° south, and the Atlantic region corresponding goes to Maldonado in 35° south. The *Cold Temperate* Regions of the two oceans cover nearly the same latitudes.

On the North American coast at Cape Hatteras, the three isocrymes 62°, 56°, and 50° F., leave the coast together; and in the Pacific on the South American coast there is a similar *node* in the system of isocrymes, the three 74°, 68°, and 62°, proceeding nearly together from the vicinity of Cape Blanco.

Viewing these regions through the two oceans, instead of along the coasts, other peculiarities no less remarkable are brought out. The average breadth of the *South Torrid* Region in the Pacific, is more than twice as great as that of the same in the Atlantic; and the most southern limit of the latter is five degrees short of the limit of the former in mid-ocean. So also, the *Subtorrid* Region at its greatest elongation southward in the Atlantic, hardly extends beyond the mean course of the line of 68° F. in the Pacific, and the average breadth of the former is but two-thirds that of the latter. The same is true to an almost equal extent of the Warm Temperate and Temperate Regions.

The breadth of the Torrid Region of the Pacific to the eastward, where narrowest, is about six degrees; and to the westward, between its extreme limits, forty-nine degrees. The Torrid zone or Coral-reef Seas, in the same ocean, has a breadth near America, of about eighteen degrees, and near Australia and Asia, of sixty-six degrees.

New Zealand lies within the Subtemperate and Cold Temperate Regions, excepting its southern portion, which appears to pertain like Fuegia to the Subfrigid. Van Diemens Land, exclusive of its northern shores, is within the Cold Temperate.

Other particulars respecting the temperature regions through the Pacific will be gathered from the Chart.

Indian Ocean Regions.—The Torrid Region covers the larger part of the Indian Ocean, including all north of the equator, and embracing the larger part of Madagascar. The Subtorrid extends just beyond Port Natal on the African coast (four degrees of latitude north of Cape Town), where there are coral reefs, and also covers the northern part of the Red Sea. The Warm Temperate and Temperate regions each claim a part of the South African coast, and the latter terminates at Cape Lagulhas.

It hence follows that Port Natal, in latitude 30° south, the Hawaiian Islands, and Bermudas, lie within regions of the same name; while Cape Town, in latitude 34° south, is in a like region with northern New Zealand, Valparaiso, the Atlantic shores of Portugal, and the sea between Cape Hatteras and Cape Cod.

The areas of the Torrid, Temperate, and Frigid zones of ocean temperature, either side of the equator, considering 27° as the normal limit between the first two of these zones, and 56° the limit between the Frigid and Temperate, are as follows:—

Torrid zone,	33,711,200	square	miles	(geographical)
Temperate zone,	27,849,500	"	"	"
Frigid zone,	12,694,700	"	"	"

It is hence seen that the Temperate zone, although two degrees wider than the Torrid, has not as large a surface. The species of marine life, if distributed equally over the two, would, therefore, be one-fifth more numerous in the Torrid zone than in the Temperate, unless the extent of ocean and coast line were far greater in the Temperate than in the Torrid zone, which is not the case. The ocean in the southern Temperate is much more extensive than that of the southern Torrid; but the coast line is far less extensive in the former, as it does not abound in islands like the Torrid zone.* It is difficult to fix upon exact ratios and we do not attempt it.

* The following table gives very closely the surface of the zones in square geographical miles, for every 2½ degrees of latitude to the parallel of 60°: it is deduced from a larger table by Berghaus, in his Länder- und Völker-kunde, i. 47. The first is the zone from the equator to the parallel of 2½°, the second, from 2½° to 5°, and so on.

2½°	3,239,296	32½°	2,769,696
5°	3,232,800	35°	2,693,760
7½°	3,220,496	37½°	2,612,688
10°	3,202,048	40°	2,526,624
12½°	3,177,472	42½°	2,435,776
15°	3,146,912	45°	2,340,256
17½°	3,110,320	47½°	2,241,280
20°	3,067,808	50°	2,136,128
22½°	3,019,472	52½°	2,027,840
25°	2,962,176	55°	1,915,696
27½°	2,905,632	57½°	1,767,168
30°	2,840,368	60°	1,680,704

The zone from 60° to 70° has the area, . . . 5,466,992
" 70° to 80° " " . . . 3,350,064
" 80° to 90° " " . . . 1,128,144

The range of temperature is far greater in the Temperate zone than in the Torrid, it being 20° F. in the latter, and 33° F. in the former; and this should be a cause of a greater variety of genera in the latter for the same number of species.

In the Torrid zone, the *Subtorrid* Region has nearly one-third the surface of the *Torrid* Region, and not one-fourth as much coast line, facts which should be regarded in comparing the number of species of the two.

We add here, a few brief remarks, in a popular way, on the origin of the peculiar forms and positions presented by the isothermal lines of the ocean. The great currents of the globe are admitted to be the causes that produce the flexures and modify the courses of these lines. These currents are usually of great depth, and consequently the deflecting land will be the deeply seated slopes off a coast, beyond ordinary soundings.

The *eastern* coasts of the continents either side of the equator, feel the influence of a warm equatorial current, which flows westward over each ocean, and is diverted north and south by the coasts against which it impinges, and more or less according to the direction of the coast.

The *western* coasts of the continents, on the contrary, receive a strong extra-tropical or polar current. In the southern oceans, it flows from the westward, or southward and westward, in latitudes 45° to 65° south, and is brought to the surface by the submarine lands or the submarine slopes of islands or continents: reaching the continents of Africa and South America, it follows along the western coasts towards the equator. The same current, being divided by the southern cape of America, flows also, with less volume up the eastern coast, either inside of the warmer tropical current, or else on both sides of it. In the Northern Seas, the system of polar currents is mainly the same, though less regular; their influence is felt on both eastern and western coasts, but more strongly on the *eastern*. In the Atlantic, the latter reduces the temperature of the waters three or four degrees along the north coast of South America, as far nearly as Cape St. Roque.

The cold currents are most apparent along the coasts of continents and about islands, because they are here brought to the surface, the submarine slopes lifting them upward, as they flow on. The limits of their influence towards the equator depends often on the bend of the coast; for a prominent cape or a bend in the outline will change the exposure of a coast from that favorable to the polar current to that favorable to the tropical, or the reverse. Thus it is at Cape Hatteras, on the coast of the United States; Cape Verde, on Western Africa; Cape Blanco, on western South America, etc.

These are important principles modifying the courses of the oceanic isothermal lines. We may now proceed to the application

of them which the best authors afford us, and to some conclusions flowing from the facts.

In the Atlantic, the warm tropical current flowing westward, is trended somewhat northward by the northern coast of South America, and still more so by the West India Islands, and thus it gradually curves around to parallelism with the coast of the United States. But south of Newfoundland, either wholly from the influence of the colder current with which it meets, or in part from meeting with submarine slopes that serve to deflect it, it passes eastward, and afterwards, where it is again free to expand, it spreads both eastward and north-eastward. The flexures in the isocrymes of 74° and 68° F., near the United States coast, thus have their origin. For the same reason, the line of 56° F. is nearly straight, till it reaches beyond the influence of the Newfoundland Banks, and then makes its Gulf Stream flexure. The line of 44° F. for the same reason,—the spreading of the Gulf Stream waters—diverges far from the equator in its easterly course, and even rises in a long loop between Great Britain and Iceland.

The cold currents flowing down the eastern coast of America, bend the isocrymes far south close along the coast, and make a remarkable southern flexure in the isocrymes of 68° F. outside of the Gulf Stream flexure. So on the western coast of Britain, the isocryme of 44° F. has a deep southern flexure, for a like cause.

The waters of the tropical current gradually cool down in their progress, through the influence of the colder waters which they encounter; and along the isocryme of 62°, they have in the colder seasons a common temperature with that of the ocean, so that the course of the Gulf Stream is but faintly marked in it. And also in the western half of the region covered by the isocryme of 56°, the colder and warmer waters have reached this as a mean temperature. Owing to the influence of the polar current on the northern coast of South America, the equator of heat lies at a distance from the land.

Up the western coast of Africa flows the cold current from the south and west, bending upward all the isocrymal lines; and passing north of the equator, it produces a large southern bend, off the coast of Africa, in the northern isocryme of 74° outside of the warm current flexure from the coast of Guinea, and also a large northern flexure in the heat-equator.*

The Atlantic tropical current also flows in part down the eastern coast of South America, giving a deep flexure to each of the isocrymes, besides making these lines to diverge from the equator, through all their length. Again, the polar current passes north-

* Along the ocean, near Africa, south and southeast of the Cape Verdes, Captain Wilkes found a current setting to the northward for much of the time until passing the equator.

ward nearer the coast-line, bending far back the western extremity of each of the isocrymes.

In the Pacific, the *tropical* currents show their effects near the coasts of New Holland and China, in a gradual divergence of the lines from the equator. The ranges of islands forming the Tarawan, Radack, and Ralick Groups, appear to divert the current northward in that part of the North Pacific, and consequently the isocrymal lines bend northward near longitudes 170° west and 180°; and near Niphon, that of 68° shows a still greater northern flexure.

The influence of the *extra-tropical* currents in this ocean is remarkably great. The southern flows from the west and south, bending upward the line of 56° F. along the South American coast, producing at Valparaiso at times a sea-temperature of 48° F. Still farther north, it throws the line of 68° F. even beyond the equator and the Gallapagos; and that of 74° F., nearly fifteen hundred miles from the coast, and four hundred north of the equator. The line of 62° F. reaches even beyond Payta, the sea-temperature at this place being sometimes below 61°.

The north polar current produces the same result along the eastern coast of Asia, as on the eastern of America. The isocryme of 74° F. is bent southward from the parallel of 23° to 12° 30′ north; and that of 68° F. from 34° to 15° north, and the latter deflection is even longer than the corresponding one in the Atlantic. The trend of the coast opens it to the continued action of this current until the bend in the outline of Cochin China, below which the cold waters have less influence, although *still showing some effect upon the heat-equator*. The isocryme of 44° is bent southward to Niphon, by the same cold waters, and from this part of the northern Pacific the current appears to flow mostly between the islands of Japan and the continent.

In the Indian Ocean, the effects of the tropical current, as it flows westward, are apparent in the southern deflection of the several isocrymes. The trend of the coast favors a continuation of the current directly along the coast, and consequently, its modifying influence on the sea-temperature reaches almost to Cape Town on the coast, and passes even beyond it at sea, carrying 56° F. to the meridian of 15° east.

By comparing the regions of the different oceans, north and south of the equator, we may arrive at the mean position of the several isocrymes, and thereby discover on a grander scale, the influence of the various oceanic movements.

For the purpose of reaching mean results, the Middle Pacific is the most favorable ocean for study. This is apparent in its greater extent, and the wide distance between the modifying continents; and also no less in the greater actual regularity of the isocrymes.

We thence deduce, that the mean position of the isocryme of 74° F. is along the parallel of 20°, this being the average between the means for the North and South Pacific. In the same manner we infer that the mean position of the isocryme of 68° F. is along the parallel of 27°.

The southern isocrymes of 56° and 62° F., are evidently thrown into abnormal proximity by the cold waters of the south. This current flows eastward over the position of the isocryme of 44° F., and consequently in that latitude has nearly this temperature, although colder to the south. Hence it produces little effect in deflecting the line of 44° F.; moreover, the line of 50° F. is not pushed upward by it. But the lines of 56° and 62° F. are thrown considerably to the north by its influence, and the Warm Temperate and Temperate Regions are made very narrow. With these facts in view, we judge from a comparison of the North and South Pacific lines, that the mean position for the isocryme of 62° F. is the parallel of 32°; and for 56° F., the parallel of 37°; for the isocryme of 50° F., the mean position is nearly the parallel of 42°; for 44° F., the parallel of 47°; for 35° F., the parallel of 56°. There is thus a mean difference of five degrees of latitude for six degrees of Fahrenheit, excepting near the equator and between 35° and 44° F. These results may be tabulated as follows.*

Isocryme of	80° F.,	Parallel of	6°
"	74°,	"	20°
"	68°,	"	27°
"	62°,	"	32°
"	56°,	"	37°
"	50°,	"	42°
"	44°,	"	47°
"	35°,	"	56°

Using these results as a key for comparison we at once perceive the great influence of the oceanic movements on climate and on the geographical distribution of marine life.

The polar or extra-tropical current of the Southern Atlantic has a more northward course in mid-ocean than that of the Pacific. It consequently bears up the isocryme of 35° F. to the parallel of 50°, that is, six degrees above the mean. The effect on the other isocrymes of the Atlantic is very remarkable. We perceive, in the first place, that the *most southern* point of each of these isocrymes is not far from the mean position of the same isocrymes in the Pacific, while the most northern point of each is ten to twenty-five degrees farther north. Taking the position of the isocrymes of 68°

* We may hence deduce the temperature of those isocrymes to which the parallels of latitude for every five degrees would normally correspond. They would be for 20°, 74° F.; for 25°, 70° F.; for 30°, 64·4° F.; for 35°, 58·4° F.; for 40°, 52·4° F.; for 45°, 46·4° F.; for 50°, 41° F.; for 55°, 36° F.; for 60°, 31° F.

and 74° F., where they cross the meridian of 15° west, as the mean position for this ocean, we find that the former is eight degrees in latitude farther north than 68° F. in the South Pacific; and the mean for the latter is in 7° south, while for the same in the Pacific it is 20° south, making a difference of thirteen degrees. The effect of the cold southern waters is consequently not along the African coast alone, but pervades the whole ocean. It is hence obvious, how utterly untenable the common notion, that the tropical current from the Indian Ocean is the same which flows up the west African coast. With a temperature of 56° south of Cape Town, it would be wholly incapable of causing the great deflections for the whole South Atlantic which have been pointed out. It combines with the cold current, but does not constitute it. The facts thus sustain the opinions long since brought forward by the distinguished meteorologist Mr. Wm. C. Redfield, that the currents flowing north along the African and South American coasts are alike antarctic or cold temperate currents.*

We may now turn to the North Atlantic. In this part of the ocean, the mean positions of the isocrymes of 74° and 68° F., are near the normal positions deduced from the Pacific. The line of 62° F. is in a somewhat higher latitude, the mean position, excluding the eastern and western deflections, being near the parallel of 36°. The line of 56° F. has the parallel of 42½° north for its mean position over the middle of the ocean, which is five and a half degrees above the normal in the Pacific. The line of 50° has in the same manner for its mean position over mid-ocean, the parallel of 47½°, or again five and a half degrees above the normal position in the Pacific. The line of 44° F. may be considered as having for its mean position the parallel of 52° north, while it rises to 60° north. The lines in the North Atlantic above that of 68°, average about five degrees higher in latitude than the mean normal positions, while 68° and 74° have nearly the same places as in the Pacific. There is hence a great contrast between the Pacific, South Atlantic, and North Atlantic Oceans. This is seen in the following table containing these results:

	Normal, deduced from Pacific.	Mean position in South Atlantic.	Mean position in North Atlantic.
Isocryme of 74° F.,	20°	7° south.	21° north.
" 68°	27°	19°	28°
" 62°	32°	29°	36°
" 56°	37°	36°	42½°
" 50°	42°	39°	47½°
" 44°	47°	44°	52° (max. 60° north.)
" 35°	56°	50°	61°

The influence of the warm tropical waters in the North Atlantic lifts the isocrymes of 74° and 68° as they approach the coast of America, while the same lines are depressed on the east by the colder northern currents. Moreover, north of 68° the whole in-

* American Journal of Science, xlv. 299, 1843.

terior of the ocean is raised four to five degrees in temperature above the normal grade, by the same waters spreading eastward; and between Great Britain and Iceland, the temperature is at least ten degrees warmer than in the corresponding latitude of the South Pacific, and thirteen or fourteen degrees warmer than in the same latitude in the South Atlantic.*

The influence of so warm an ocean on the temperature of Britain, and on its living productions, animal and vegetable, is apparent, when it is considered, that the winds take the temperature nearly of the waters they pass over. And the effects on the same region, that would result from deflecting the Gulf Stream in some other direction, as brought out by Prof. Hopkins† and others, and substituting in the Northern Atlantic the temperature of the Southern Atlantic, is also obvious, without further illustration. The discussion of these subjects would be foreign to the topic before us.

The subdivision of the oceans into Temperature Regions, affords a convenient means of dividing off the coasts into *Zoological Provinces*. A comparison of the facts afforded by the distribution of Crustacea, with the positions and extent of the Provinces thus deduced, show that they are natural, and may in general be well characterized.

Zoological Provinces have been considered by some as centres of creation, and therefore of diffusion, for groups of species. But such kinds of centres we fail to distinguish in any part of the globe. Each species may have had its one point of origin and single centre of diffusion, in many, and perhaps the majority of cases: but, however the fact may be, we have no *evidence* for asserting that particular regions were without life, and were peopled by migration from specific and predetermined centres; for if there were such centres of diffusion, there are at present no means by which they may be ascertained. The particular temperature-region in which a species was created, may be ascertained by observing which is most favorable for its development; and by this course of investigation, we may find that almost every different locality has some species for which it is especially fitted, we may thus show, as far as reason and observation can do it, that all regions as a general thing, have had their own special creations.

We rather look to climatal influences, in all their various kinds, directly and indirectly exerted, and united with height or depth of site, and other geographical conditions, as giving limits to Zoolo-

* Ross, in his Antarctic Voyage, found the sea-temperature in 60° south and 3° west, 31½° F., in the month of *March;* at the South Shetlands, 61° south, the sea-temperature was 31° to 35° in January (midsummer); and in the same latitude, and 45° west, it was 30·1° in February.

† Quarterly Jour. Geol. Soc., vol. viii., p. 56, and Amer. Jour. Sci., 1853, vol. xv.

gical Provinces; and as regards marine animals, ocean-temperature is the more prominent of these influences. Under temperature, the limits or extremes are to be considered as well as the mean, and also the varying action of currents which induce the changes, especially those occasional extreme results which are of decennial rather than annual recurrence.

How far geological changes, by subsidence and elevation, have varied the distribution of the present races of animals, or given limits to zoological regions, is a point yet uninvestigated. The conclusions that have been derived from this source are mostly of a hypothetical character, and are to be received with distrust without a larger supply of evidence. It is easy to meet a difficulty by the supposition of a former union by dry land of regions now separate; but it appears to us that better evidence is needed on such a point, than those derived from the zoological fact which is to be explained.

Along the various coasts, prominent *capes* are in general the limits of Zoological Provinces; and this fact is well shown in the chart of ocean-temperature. They are, as we have explained, the points where the cold or warm currents are turned off from a coast, and where therefore there is a sudden transition in the temperature. A striking example of this has been pointed out on both the eastern coast of North America, and western of South America, where several isocrymes meet forming a kind of nodal point;—viz., Cape Hatteras, the meeting point of the isocrymes of 62°, 56°, and 50°, and Cape Blanco, the meeting point of 68°, 62°, and (nearly) 74°. So also the east cape of East Australia, is the point of meeting of the isocrymes of 74° and 68°. At the south extremity of Africa, on the west coast of Asia, there are approximations to the same fact. Cape Cod the southeast cape of New England, is a marked point in zoological geography, and the termination of the isocryme of 44° F.; and the North Cape of the La Plata inside of Maldonado is another.

We proceed to give an enumeration of the several Zoological Provinces to which we are led by the temperature regions adopted. It should be again observed, that the isocryme of 68° is the grand boundary of coral reefs, and of the larger part of the zoological life connected with them, and that the *Torrid Zone* and *Coral-reef Zone* of oceanic temperature are synonymous terms.

We mention also the extent of the Provinces; and it will be found, that although seemingly numerous, few of them are under 500 miles in length, while some are full 4000 miles.

For zoological reasons which are explained in another place,* and which may be the subject of another communication to this Journal, we adopt for *Marine* Zoological Geography, three grand

* The Author's Report on Crustacea.

divisions of the coasts of the globe. 1. The *American* or *Occidental* including East and West America; 2. The *Afrio-European* including the coasts of Europe and Western Africa; and third, the *Oriental*, including the coasts of Eastern Africa, E. Indies, Eastern and Southern Asia, and Pacific. Besides these, there are the *Arctic* and *Antarctic* Kingdoms, including the coasts of the Frigid Zones, and in some places, as Fuegia, those of the extreme temperate zone. We add here, only in general terms, that there is a remarkable similarity in the genera of Eastern and Western America, and an identity of some few species; that the coast of Europe and Eastern Africa widely differ in Crustacea from either the American or Oriental; that the species of the Oriental division have a great similarity in genera, and that numerous species of Crustacea of Eastern Africa, are identical with those of the Pacific. We pass by, for the present, the details on these points.

We also omit the zoological characters of the Provinces here enumerated. Several of these Provinces are identical with those proposed by Milne Edwards, Prof. E. Forbes, and others; and as far as possible, the names heretofore used, are retained.

I. OCCIDENTAL KINGDOM.

A. Western Section.

1. *Torrid or Coral-Reef Zone.*

Provinces.	Limits.	Length in Miles.
1. *Panamian*, (torrid) . . .	1° s. to 17½° n., . . .	1600
2. *Mexican*, Province, (N. subtorrid)	17½° n. to Californ. Penin., .	600
3. *Guayaquil*, " (S. subtorrid)	1° s. to Cape Blanco, 4¾° s., .	200

2. *North Temperate Zone.*

4. *Sonoran*, (warm temperate) .	Penin. Californ. to 28½° n., .	550
5. *Diego** or *Jacobian*, (temperate)	28½° n. to 34½° n., . .	450
6. *Californian*, (subtemperate) .	34½° n. to C. Mendocino, .	480
7. *Oregon*, (cold temperate) . .	C. Mendoc. to Puget's Sound, (?)	480
8. *Pugettian*, (subfrigid) . .	Puget's Sound to 55° or 56°,	1200

3. *South Temperate Zone.*

9. *Gallapagos*, (warm temperate) .	Gallapagos.	
10. *Peruvian*, (temperate) . .	C. Blanco to Copiapo, 27½° s.,	1500
11. *Chilian*, (subtemperate) . .	27½° s. to 38° s., . . .	700
12. *Araucanian*, (cold temperate) .	38° s to 49° or 50° s., .	900
13. *South Patagonian*, (subfrigid) .	50° s. to Magellan Straits.	

B. Eastern Section.

1. *Torrid Zone.*

1. *Caribbean*, (torrid) . . .	{ Key West & N. Yucatan to 1° south of Bahia, } .	4000
2. *Floridan*, (N. subtorrid) . .	Key West to 27° n., . .	200
3. *Brazilian*, (S. subtorrid) . .	15° s. to 24° s. . . .	600

2. *North Temperate Zone.*

4. *Carolinian*, (warm temperate) .	27° n. to C. Hatteras, . .	600
5. *Virginian*, (cold temperate) .	C. Hatterns to C. Cod, .	650
6. *Acadian*, (subfrigid)† . .	C. Cod to E. Cape of Newf'dl'd,	900

* May possibly be united conveniently to the Sonoran.
† Changed from Nova-Scotian in the Report on Crustacea.

3. *South Temperate Zone.*

Provinces.	Limits.	Length in Miles.
7. *St. Paul,** (warm temperate) .	24° s. to 30° s., . . .	480
8. *Uraguaian,* (temperate) . .	30° s. to north C. of La Plata,	360
9. *Platensian,* (subtemperate) .	Mouth of La Plata.	
10. *North Patagonian,* (cold temperate)	South C. of La Plata to 43° s.,	500
11. *South Patagonian,*† (subfrigid) .	43° s. to Magellan Straits, .	700

II. AFRICO-EUROPEAN KINGDOM.

1. *Torrid Zone.*

1. *Guincan,* (torrid) . . .	5° N. to 9° N., . . .	1200
2. *Verdensian,* (N. subtorrid) .	9° N. to 14½° N. including the Cape Verde Islands,	1000
3. The *Biafrian,* (S. subtorrid) .	5° N. to 7° or 8° S., including Ascension & St. Helena,	900

2. *N. Temperate Zone.*

4. *Canarian,* (warm temperate) .	14½° N. to 28° or 29° N. including the Canaries,	1000
5. *Mediterranean,* (temperate) .	29° N. to C. St. Vincent with Mediterranean, excepting some of its northern coasts, and including Madeira and Azores.	
6. *Lusitanian,* (subtemperate) .	C. St. Vincent to 42° N., .	300
7. *Celtic,* (cold temperate) . .	42° N. to Scotland, . .	1000
8. *Caledonian,* (subfrigid) . .	N. Scotland, Shetland's Ferroe, etc.	

3. *South Temperate Zone.*

9. *Angolan,* (warm temperate) .	7° S. to 13° S., . . .	360
10. *Benguelan,* (temperate) . .	13° S. to 28° S., . .	900
11. *Capensian,* (subtemperate) .	28° S. to C. Agulhas, .	450
13. *Tristensian,* (cold temperate) .	Tristan d'Acunha.	

III. ORIENTAL KINGDOM.

I. African Section, or East Coast of Africa and Neighboring Islands.

1. *Abyssinian,* (torrid) . . .	26½° S. to 21° or 22° in Red Sea, including larger part of Madagascar and Islands north,	3500
2. *Erythrean,* (N. subtorrid) . .	Northern third of Red Sea, about	300
3. *Natalensian,* (S. subtorrid) . .	26½° S. to 31° S., with Southern Madagascar and Isle of France.	
4. *Algoan,* (warm temp. and temp.)	31° S. to C. Lagulhas, .	550

II. Asiatic Section.

1. *Torrid Zone.*

1. *Indian,* (torrid)	East India Islands, N. Australia, Southern Asia, to 12½° N. on Cochin China.	
2. *Liukiuan,* (N. subtorrid) . .	12½° N. to 15° N., with Formosa, Loochoos, S. S.E. Shore of Japan.	
3. *Endrachtian* or W. Australian, (S. subtorrid)	W. Australia 22° S. to 26½° S.,	300

* The St. Paul province may perhaps be united with the Uraguaian.

† The South Patagonian is made to include both the eastern and western sides of this portion of the continent; but a division of the two may hereafter be found to be required.

2. *North Temperate Zone.*

Provinces.	Limits.
4. *Tonquin*, (warm temperate) . .	15° N. to 25° N., (Gulf Tonquin.)
5. *Chusan*, (subtemperate) . . .	25° N. into Japan Sea.
6. *Niphonensian*, (cold temp. & subtemp.)	East Coast of Niphon, to 40° N.
7. *Saghalian*, (subfrigid) . . .	Coast of Japan Sea, part of Western and Northern Niphon, Saghalian, Yeso, etc.

3. *South Temperate Zone.*

8. *Cygnian*, or Swan R. (warm temperate)	W. Australia, 26½° s. to S. W. Cape.
9. *Flinders*, (temperate) . . .	Southern Coast of Australia.
10. *Moreton*, (warm temp. and temp.) .	E. Australia, 26½° s. to 31° s.
11. *Bass*, (subtemperate) . . .	E. Australia, 31° or 32° s. to Van Diemens Land.
12. *Tasmanian*, (cold temperate) . .	Van Diemens Land.

III. Pacific Section.

1. *Torrid Zone.*

1. *Polynesian*, (torrid)	Pacific Ids. of Torrid Region.
2. *Hawaiian*, (N. subtorrid) . . .	Hawaiian range of Islands.
8. *Raratongan*, (S. subtorrid) . .	Hervey Islands and others of South Subtorrid Region.

2. *South Temperate Zone.*

4. *Kermadec*, (warm temp. and temp.) .	Kermadec Islands, etc.
5. *Wangaroan*, (subtemperate) . .	Northern New Zealand.
6. *Chatham*, (cold temperate) . .	Middle N. Z. to 46° S. and Chatham I.

The Arctic Kingdom includes (1) the *Norwegian*, north of the Atlantic,—(2) the *Camtschatican*, north of the Pacific,—(3) the *North Polar*; the Antarctic Kingdom, includes (1) the *Fuegian*, Fuegia and Shetlands, etc.,—(2) the *Aucklandian*, Auckland and S. extremity of New Zealand,—(3) the *South Polar*.

Art. XXXII.—*The Coal Field of Bristol County and of Rhode Island;* by President E. Hitchcock.*

In my Reports on the Geology of Massachusetts, I described a large tract in Bristol county, a part of Plymouth county, the whole of the island of Rhode Island, and a strip on the west side of Narraganset Bay, as underlaid by Graywacke; a rock older than the Coal Formation, and equivalent to the Silurian and Cambrian strata of late geological writers. While publishing my Final Report, however, in 1840, I became satisfied that a part of this region was a true coal formation, and so marked it on the map. But I now advance a step farther, and maintain that the whole of this tract, embracing not less than five hundred square miles, is a genuine coal field, that has experienced more than usual metamorphic action.

* From a Report to the Governor of Massachusetts, dated Feb. 23, 1853.

The metamorphic action to which this deposit and the coal have been subject, is two-fold: first, *mechanical*, secondly, *chemical.*

The mechanical force seems to have operated upon the strata containing the coal in a lateral direction, so as not only to raise them into a highly inclined position, but also to produce plaits, or folds, such as would be formed if several sheets of paper, lying upon one another, were taken into a man's hand, and by pressure on the opposite edges, were crumpled so as to form ridges and hollows.

* * * * * *

This same effect has been produced upon the coal strata of Massachusetts and Rhode Island, as well as in the great Appalachian coal field of Pennsylvania and Virginia. The miners are familiar with these irregularities, and they constitute some of the most serious difficulties with which they meet. In the mine of the Blackstone Coal Company, at Valley Falls, R. I., for instance, the bed, and the accompanying rocks, which for a considerable distance dip nearly 45° to the *northwest*, begin to curve so as at length to dip southeasterly.

In the Aquidneck mine, at the north end of Rhode Island, the bed of coal that had been worked, was in some places pinched up to a width of not more than one or two feet, while in other places it has a thickness of from ten to fifteen feet. I noticed here also, another peculiarity, which it is difficult to describe intelligibly without drawings. It is this; that the foldings do not run either perpendicular or parallel to the strike (direction) of the strata, but obliquely. Hence, when a thick mass of coal is reached, it passes into the earth between the strata in an oblique direction, and other expansions (as well as the contractions) of the bed, will run parallel to it. This fact will be of some practical value, but, perhaps, its chief interest lies in its scientific bearings.

Another effect of this plicating force has been to crush the coal, so that when dug out, it crumbles too much, and produces a great deal of fine coal. Sometimes the coal appears as if it had been severely squeezed and made to slide along between the strata. I find it to be a common impresion among those familiar with these mines, that the force came from the northwest. But in the Aquidneck mine, the direction of the folds (somewhat southeasterly,) would indicate a pressure either from the N. E. or S. W.

The chemical metamorphosis which these rocks have experienced, consists mainly in such effects as heat would produce. By an inspection of the map appended, it will be seen that this coal basin is in a great measure surrounded by unstratified rocks, such as granite and syenite,—which all geologists now admit to have been once melted. I have never seen any example in which they have sent veins into the coal strata: a fact that seems to indicate that the latter were not deposited till after the granite and syenite

were partially cooled. Yet along the margin of this coal field, especially at Newport, all along the west shore of Narraganset Bay, at Smithfield, Cumberland, Wrentham, Bellingham, &c., we find the slates very much metamorphosed, so as only heat could have done it. I infer, therefore, that these slates were deposited while great heat yet existed in the subjacent unstratified rocks; and although the coal strata may be newer than the metamorphic slates, yet they also show a greater degree of induration, and in some instances more of crystallization than is common in undisturbed coal fields; and cross seams, or divisional structures, are also more common than usual. These too, are now usually referred to the action of heat. The coal, also, from this basin, has a greater specific gravity than most anthracite; bearing a proportion to the Pennsylvania anthracite, of 1·75 to 1·55. It has a tendency greater than usual, to break into cuboidal fragments, and has more the shining aspect of crystalline minerals. I ought to add, that in some of the mines, the metamorphic action has been so great as to obliterate all traces of organic remains. This is so remarkably the case at a coal bed, seven feet thick, in Worcester, that no trace of a vegetable has to my knowledge ever been discovered there, and the anthracite has a still more stone-like aspect, is heavier than that from Rhode Island, and is partially converted into plumbago. Yet, even this, most geologists would not hesitate to pronounce a genuine coal formation.

Such are the chief circumstances that have so long perplexed the geologists in respect to the age of the coal of Rhode Island and Massachusetts, and made them hesitate whether to regard its age as precisely equivalent to that of the regular coal formation. But at length the evidence is too strong to resist, that this deposit belongs to a true coal field, and is of the same age as the great coal deposits in our own country in Pennsylvania, Virginia, Ohio, Michigan, Illinois, and Iowa; and those of England, Scotland, France, and Belgium. The only difference seems to be, that this is a metamorphic coal field.

Such an identification of this deposit is a point of great importance in forming a judgment of its value, and it therefore seems desirable that the evidence should be presented. This will involve a description of all the important facts with which I have become acquainted respecting this coal field.

I. *In the first place, the general outline of the surface over this field, corresponds with that of a regular coal field, or basin.* It is generally nearly level, save some gentle swells and a few outliers of rock, the remnants of former more extensive masses. On its margin the older and more crystalline rocks rise higher, though not very much so; for there is reason to think that both the margin and the surface of the coal field have been subject to powerful denudation.

By this argument I intend merely to show that there is no improbability in the supposition that this basin of five hundred square miles, may be a coal field; just because it looks like one. But the argument has no great force; because other deposits may exhibit a similar surface. Strictly speaking, it merely shows that the surface is underlaid by one formation, or by closely allied formations.

II. *The rocks correspond essentially to those of the coal measures.* The predominant varieties are four: 1. A dark colored slate, or slaty clay, often much indurated, and more or less charged with carbon, lying in immediate contact with the coal, especially beneath it. Its surface is sometimes highly glazed, as if by heat, or friction. I noticed fine specimens at the Roger Williams mine, a mile or two north of Valley Falls, Rhode Island. 2. A coarse light gray grit, or sandstone, lying, so far as I have observed, immediately above the coal beds, and easily disintegrating at the surface. 3. A dark gray, hard grit, or sandstone: a much more extensive rock, forming in fact the principal one between the beds of coal, and in some places embracing coal without the intervention of shale. 4. A coarse gray conglomerate, which probably underlies the other rocks above described, and may be the equivalent of the millstone grit that forms the basis of other coal fields. I do not feel satisfied, however, that such is always the position of this rock. Further examination is needed. This rock occurs in various places along the eastern part of Massachusetts; but nowhere, that I know of, associated with coal, save in the Bristol coal field.

Several other varieties of rocks exist on the borders of this coal field, but whether they are the coal measures metamorphosed, or older rocks, such as the Devonian and Silurian, is not certainly known. They consist of gray and red slates, and red sandstones and conglomerates. As to the red varieties, I have little doubt but that they belong to the Devonian or old red sandstone system, and have accordingly so represented them where they are most fully developed, viz.: in Wrentham and Attleborough. In the south part of the former town, this red rock forms a striking feature in the landscape. I found it there, by the Aneroid barometer, to rise about four hundred feet above the general surface at the Mansfield coal mines, and three hundred feet above the excavation once made in Wrentham for coal. On this hill I found the red rock to have a strike E. 10° N., and a dip 70° southerly; so that it must pass beneath the coal field, as we should expect if it were the old red sandstone. A mile south of the meeting-house in North Attleborough, however, where this red rock (here mostly a conglomerate) shows itself, the strike is E. 15° N., and the dip 35° northerly. This fact looks as if we might here be on the southern border of a coal basin, and at Wrentham on the northern border, and that there may be an axis of the older rocks running

northeasterly across the coal field. But this point needs further examination, and I have not given such a view of the matter on the map. But I thought it best to indicate the region where I have seen the old red sandstone, as I presume to call it, most fully developed.

Now, setting aside these border rocks, and looking only at those in connection with the coal, I think we cannot hesitate to identify them with those of other coal fields; though, perhaps in Massachusetts and Rhode Island rather more metamorphosed than is common.

III. *The number, position, strike, dip, and general character, of the beds of coal, already discovered in the district under consideration, render it probable that it is all one coal field, or essentially one.*

Under this head I am led to describe briefly all the coal beds in this deposit known to me, most of which have been explored to a greater or less extent. I have visited them all, with one or two exceptions of no consequence; and descended into all the mines that are accessible.

1. *Beds of coal in Mansfield.*—These have been opened in two parts of the town. One is near the centre where a shaft was sunk by the Mansfield Coal Company, some fifteen years ago, sixty-four feet; but only a little coal was found.

About the same time, the Mansfied Mining Company sunk a shaft eighty-four feet near the Hardon farm, two miles southwest of the centre. A drift was then carried across the strata, and it is said that seven beds, of various thickness, up to ten feet, were found. Dip of these beds, 53° N. W. Strike, S. W. and N. E.

More recently, in 1848, I believe, the Mansfield Coal and Mining Company, through the enterprise and perseverance of B. F. Sawyer, Esq., sunk a shaft near the same place, one hundred and seventy feet, and ten feet in diameter: from which, according to the statements of Thomas S. Ridgway, Esq., the engineer, they have carried a south tunnel six hundred and sixty feet, and other tunnels and gangways to about the same amount. Not less than thirteen beds of coal have been crossed, but none of them thick. They are very irregular, sometimes swelling out to six or eight feet in thickness, and then pinched up to a few inches. The dip varies from 30° to 70° N. W., and the strike is nearly N.E. and S. W. Although these excavations are not far from the old Hardon mine, the beds are said to have little correspondence.

2. *Bed in Foxborough.*—This is only about two miles from the Mansfield beds, and two excavations were made there several years ago, and good coal obtained; but the pits are filled up so that I could not ascertain the strike, dip, and width of the bed.

3. *Beds in Wrentham.*—In the south part of the town, a pit was sunk many years ago, about one hundred and eighty feet, mostly in dark carbonaceous slate, and several beds found. The

coal which I have seen from this spot is not good, having forty per cent. of ash. Strike of the bed, nearly E. and W.; dip, 45° N.

4. *In Raynham.*—An outcrop of coal appears in this town, about three feet thick, which has not been explored, except a few feet. Strike, N. 50° E., dip, 45° S. E.

5. *In Bridgewater.*—Indications of coal were shown me from the rock thrown up in digging a well in the south part of the town, but nothing further could be learned.

6. *In Taunton.*—Two miles northwest of the town, a similar opening was shown me, but I could not learn the dip and direction of the slate. Four miles to the west of the town, I was told that similar indications existed. The same is true of West Bridgewater; and in Berkley coal plants are found, such as usually accompany beds of coal.

7. *In Cumberland, Rhode Island.*—This is called the Roger Williams mine, which was opened many years ago; but the works were burnt, and the explorations abandoned. But they have been resumed within a few years, under the superintendence of Capt. Thomas Martin. A shaft has been sunk three hundred feet perpendicularly, into which I descended, with Capt. Martin. The old bed, whose strike was nearly N. E. and S. W., has been abandoned, and by carrying a horizontal drift two hundred and sixty feet, a new bed was struck, which, at the place, runs nearly N. and S., and dips west about 45°. The average width was stated to be fifteen feet, and in some places twenty-three feet. If this be not a mere protuberant mass, occasioned by lateral pressure, it indicates a larger amount of coal than I have seen in any other mines in this coal field.

8. *The Valley Falls Mine.*—This is scarcely more than a mile south from the Roger Williams mine: yet the strike of the beds will not allow us to suppose them connected. The operations here are carried on by the Blackstone Coal Company. A shaft is carried down, which follows a bed of coal, with dip near the surface of 30° to 45°. This bed, which I examined several years ago, to the depth of about fifty feet, exposed a thickness of coal from six to nine feet, and the direction was N. 50° to 60° E. Since that time, the "incline," as the miners call it, has been pushed downward five hundred feet, or about three hundred and seventy-five feet of perpendicular depth, and, as already mentioned, the strata have been found to curve very much, and not less than five beds of coal have been crossed; the best having a width of six feet. One nest of coal was found thirty feet square.

9. *In Seekonk.*—I am informed by J. N. Bollies, Esq., of Providence, that the outcrop of a bed of coal was found in digging a well in this place, only fifteen feet from the surface. Its quality was similar to that found at Valley Falls. In the same region occur very fine specimens of coal plants, especially Calamites.

10. *In Providence.*—The same gentleman, in boring for water, in the north part of Providence, at the depth of sixty feet, struck a bed of coal dipping N. E. 45°, which is ten feet thick, and of the same general character as that at Valley Falls, which is known to burn well.

11. *In Cranston, R. Island.*—This town is on the west side of Narraganset Bay, along which the coal rocks extend as far as Warwick. In Cranston, according to Dr. C. T. Jackson, "slate, graphite, and impure anthracite," are found in an excavation seven or eight feet deep. Coal plants are very abundant on Warwick Neck; but no coal has been found.

12. *In Bristol, R. I.*—The coal bed in this place is in the west part of the town, and the spot where it crops out is only a few feet above the harbor. It was discovered in sinking a large well. Although I descended into it, I could not ascertain the thickness of the bed, nor with accuracy its strike and dip. Approximately it runs N. a few degrees E., and dips westerly about 48°. The coal did not appear to me to be as much crushed as in some mines, and seems of an excellent quality.

13. *Portsmouth Mine, or Case's Mine, in Rhode Island.*—This mine, situated in the northeast part of the island of Rhode Island, was opened in 1808, which was earlier than the Pennsylvania mines were explored. At that time the mode of burning anthracite was not known, and the coal was not sought after, and the work was abandoned in 1813 or 1814. Some years afterwards it was resumed, and in 1827, according to Mr. Clowes, the agent, twenty men and five boys raised about forty-four hundred tons of coal, coarse and fine. But the work was again abandoned not long after, and not resumed till 1847, when the mine was opened by the Portsmouth Coal Company, which has also ceased operations there. Mr. Barbour speaks of the amount of coal and rock, "principally the former," that has been excavated at this place, as about one hundred thousand tons.

Three beds were discovered at this place, "all of workable width." Dr. C. T. Jackson says that the bed last wrought was thirteen feet thick. He states its strike to be S. 80°, W. and N. 80° E.; dip 35° southeasterly. Mr. Clowes, however, says that the beds run N. E. and S. W., and dip from 40° to 90° southeast. As the mine is now unwrought I could not settle these points.

14. *The Aquidneck Mine.*—This is in Portsmouth, and only about two miles west of Case's mine. Indeed there is reason to suppose, that while the latter occupies the northeast side, the former is situated upon the northwest side of the same coal basin. The strike of the strata and coal beds is N. a few degrees E., and the dip of the bed now wrought, varies from 28° to 35° S. E. Three beds of coal occur, only one of which has been wrought by the Aquidneck Coal Company, which bed, I was told, varied

in width from two to twenty feet. They have followed down the middle bed to the depth of six hundred and twenty feet, from which six gangways have been extended, from eighty to eight hundred and forty-four feet each. During the last half of 1851, thirty-one hundred tons of coal were taken out, and an opening is now made into a subjacent bed. The appearance of the operations at this mine, under the superintendence of Arad Gilbert, Esq., and the quality of the coal, appeared to me more satisfactory and promising than at any other mine in this coal field.

The mouth of this mine is not more than twenty feet above the ocean, and only a few rods distant; yet, after descending about one-third of the distance, to the bottom of the inclined shaft, I found the mine quite dry, although at the bottom, I was some hundreds of feet below the waters. The coal appeared to me less crushed at this mine than at most others; yet the bed is very unequal in width; showing that the folding agency has here operated.

The opening of this mine is several hundred feet lower than that of the Case mine. The latter, however, I have understood to be subject to the influx of water, but I cannot learn how deep it was ever wrought. I consider the question whether these two mines are opened on the same bed, as yet quite unsettled. Both of them lie near to protruding mines of granite, and have been subject to a good deal of disturbance.

15. *In Newport, R. Island.*—In the southeast part of this town a thin bed of coal shows itself on the coast, where the dark strata of slate stand nearly perpendicular, and have a direction nearly east and west. During the revolutionary war, the British made some excavations at this spot, in the hope of finding fuel. But the prospect is very poor, especially as the spot is so near granite, which has affected the strata. Yet at this place the shale abounds in coal plants.

Let me now present, in a tabular form, the leading facts respecting these fifteen localities, where coal has been discovered in this field; at least in those cases where the facts are definite enough to tabulate.

Locality.	No. of Beds.	Thickness of do.	Strike of do.	Dip.
Mansfield, Centre, .	1	A few inches,	N. E. and S. W.,	N. W., large.
" Hardon,	7	10 feet,	N. E. and S. W.,	53° N. W.
" New, .	13	7 feet max.,	N. E. and S. W.,	30° to 70° N. W.
Wrentham, . . .	1	– –	E. and W.,	45° N.
Raynham, . . .	1	3 feet,	N. 50° E.,	45° S. E.
Cumberland, . .	2	15 to 23 feet,	N. E. & S. W. & N. & S.,	45° W.
Valley Falls, . .	5	6 to 9 feet,	N. 50° to 60° E.,	30° to 45°
Providence, . . .	1	10 feet,	N. W. and S. E.,	45° N. E.
Bristol, ...	1	– –	N. somewhat E.,	48° N. W.
Portsmouth, Case's,	3	13 feet max.,	N. E. and S. W.,	40° to 90° S. E.
" Aquidneck,	3	2 to 20 feet,	N. a few degrees E.,	28° to 35° S. E.

The geologist will notice two circumstances of importance in the preceding facts; one is, that there is a tendency to a northeast and southwest direction in the beds of coal. The other is, that where horizontal drifts of any considerable extent have been carried across the strata, several beds have been crossed. The exceptions in the dip can easily be explained by the proximity of the older crystalline rocks, which may have greatly changed the direction, at least for some distance; and the opposite dips observable in some cases, may be referred to the same cause, or perhaps to an anticlinal axis of older rocks, which I have some reason to suspect, may cross this coal field in a N. E. and S. W. direction.

It seems to me difficult to avoid the conclusion, that these different beds, scattered as they are so widely, all belong to one and the same coal field; although denudation or vertical movements may have rendered some parts of it much less productive than others. The strike and dip, as well as the number of beds, correspond as well to other coal fields as we ought to expect, when we consider the great amount of metamorphic action which has here been exhibited.

It ought to be known, too, in this connection, that this region is densely covered by accumulations of drift, and the sand and gravel of ancient sea beds. Especially is this the case where the rock is the softest,—that is, the black shale,—which contains the coal beds. Hence rocks rarely show themselves at the surface, and the wonder is, that so many beds of coal have been discovered, rather than that no more have been found. The digging of wells and other excavations have been the principal means of bringing them to light; nor will any reasonable man doubt that probably many more are concealed beneath so thick a coating of drifted materials.

IV.—*The character of the vegetable remains found in connection with these coal beds, make it almost certain that they belong to the coal measures of the carboniferous system.*

[Figures of species of Stigmaria, Calamites, Pachypteris or Odontopteris, and Neuropteris, on two plates, are here referred to.]

I might add several other species of plants peculiar to the coal formation, and found in this field. But it seems to me unnecessary. Those already exhibited appear to settle the question as to the true place of these deposits, in the geological scale. No geologist would think of putting them in any other part of the series than the carboniferous. I have not, indeed, met with any specimens in these rocks, of Sigillaria and Lepidodendra, which are common in the coal rocks of Pennsylvania and Ohio. But it is more easy to explain their absence from a real coal field, than the presence of so many other plants, identical with those of the coal measures, in any other formation.

In view of all these proofs I am led to the inference that this is a genuine coal field of the carboniferous series. And the tendency to a northeast and southwest direction in the beds, as well as the high dip, leads strongly to the conclusion, that this is only a detached portion of the great Appalachian Coal Field, which stretches through the Middle States.

In comparing the character of this deposit with those of European coal fields, I am led to regard it as very similar to the *Upper Culm Measures* in Devonshire in England. These are composed of "sandstones and indurated shales, (the latter containing the Culm or Anthracite,) and are of great but unascertained thickness, being perpetually interrupted, coiled upon themselves, and repeated over again, forming an incredible number of anticlinal and synclinal lines." (*Ansted.*) A considerable number of beds of coal occur in these rocks which are several feet thick, and they are more or less extensively wrought. Only a few years ago, some of the ablest English geologists (De la Beche and others) contended that these rocks were not true coal measures, but considerably lower in the series. Yet I believe they are now universally regarded as belonging to the carboniferous system, and on the same evidence as I have just adduced, viz.: the organic remains. The Devonshire beds seem to be more changed by metamorphic action than those of the Bristol coal field, if we may judge from the fact, that the vegetable impressions are more distinct here than in England.

In Brittany, Normandy, le Maine, and Anjou, in France, according to the French geologists, analogous rocks occur, containing beds of anthracite more than three feet thick. And though referred by some to the Graywacke strata, they are at last taking their place in the true carboniferous system, as are also the anthracite strata of the Alps, which have been regarded as newer than the coal formation. (*See Quarterly Journal of Geology for August*, 1851, p. 91.)

The evidence, then, seems very strong, on which I base the conclusion that the Bristol and Rhode Island deposits, with vegetables remains, possess the age and characters of a true coal field as the carboniferous period of the geologists. And if it be indeed so, much greater confidence of success may be entertained in the researches after coal, which have for some years been making, than if we could assign no settled place to the rock, or that place were higher or lower than the carboniferous system. * * *

Art. XXXIII.—*Researches on Different Applications of Magnetic Attraction;* by M. J. Nicklès, Doctor in Science.

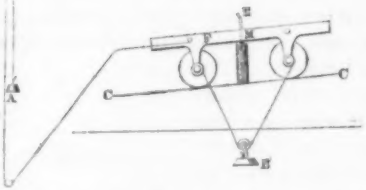
Part I.—*Use of Magnetic Attraction in Locomotion on Railroads.*

The researches here described, have been in progress three years. Their object has been to give to the driving wheels of locomotives a power of adhesion independent of pressure or weight, an adhesion consequently that will not vary with the grade, and which at the same time may be suspended or brought into action at will.

My first experiments were made in conjunction with a Paris mechanician, M. Amberger, who had called my attention to this subject by consulting me as to the means which physical science could furnish toward it. My first idea was to make the driving wheels electro-magnets; but not finding in known facts sufficient data for magnetizing a wheel in motion, I undertook provisionally to use an electro-magnet acting, at a distance, upon the rail, and fixed upon a support moving upon the railroad.

This apparatus consists of a framework of iron, F, (fig. 1.) moving on 4 wheels in pairs, to which a rotatory movement is imparted by means of a weight B, acting on the wheels in place of steam. Another weight A, fixed at the extremity of the car by means of a cord, represents the resistance, or the charge to be drawn.

1.



This car was placed on a railroad C, whose grade could be varied. At M it carried a horse-shoe magnet E, placed across the track, and so that its arms were separated by the distance between the rails. This electro-magnet, one arm of which only is represented in section in figure 1, consists of a bar of iron 1 centimeter in section, bent into a horse-shoe form, and having each arm furnished with about 250 turns of copper wire 1 millimeter thick. The poles of this magnet were about 4 millimeters from the rail on which they were to act. The pile consisted of two of Bunsen's elements as modified by Deleuil.

The first experiment was made on a grade of 80 millimeters to the meter. The car was put on the rails and the weight B left to fall by its gravity, and so give motion to the wheels about which the cord was wound. Under this force the car tended to pass over the rail, and motion began when the weight A was removed; although the adhesion was sufficient for transporting its own weight, it was not enough to balance the resistance exerted by A, (A representing the train to be drawn.) The addition of the electro-magnet was intended to give to the driving wheels

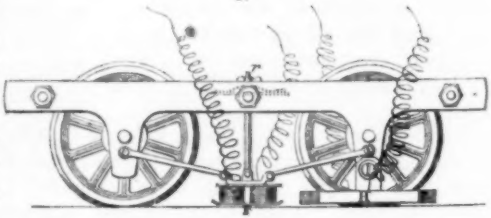
this supplement of adhesion, and in fact, on closing the circuit, the car passed over the rails without difficulty; on breaking the connection again, it stopped and fell back on the track. The weight A used in the experiment was two kilogrammes, and the motor weight B about six. The figure is reduced to one-fifth the lineal dimensions.

In this experiment, the adhesion derived from the weight of the engine was obviously replaced by another force differing in being independent of the grade of the track, since the magnetic attraction always acts perpendicularly to the plane of the armature whatever may be the position or inclination, while gravity acts at right angles to a true horizontal plane. The constancy of the magnetic adhesion is proved by means of a piece of apparatus which is described below.

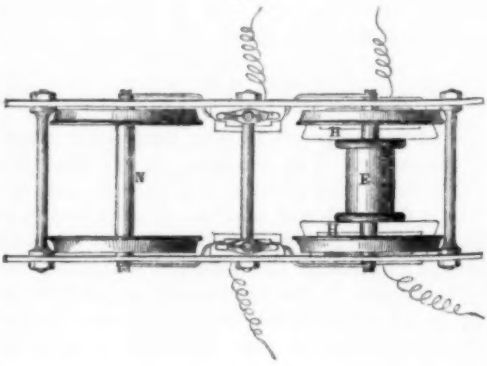
Although this experiment was so simple and unpretending, it afforded an encouraging demonstration of the problem in view, and it has interested the different professors of physics of Paris, MM. Despretz, Becquerel, Silbermann, etc., who have exhibited it in their courses, as well as the other experiments below detailed.

The process just explained would certainly have been very simple, if the successful application on a large scale had required only that an electro-magnet should be placed on each side of the driving wheels. But this method would not stand the most elementary considerations. The attraction diminishes as the square of the distance; and moreover the magnetic adhesion not exceeding $\frac{1}{10}$ the power of the magnets at 4 millimeters distance,—since the adhesion is, at a mean, $\frac{1}{10}$ of the pressure—the required force would not be realized even with batteries of great power.

2.



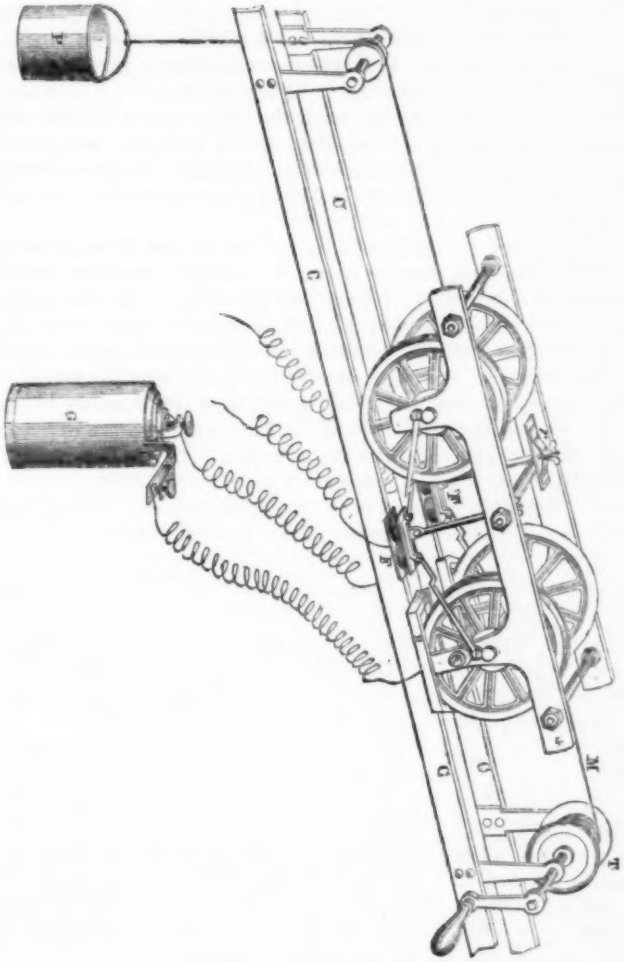
3.



The only possible method seemed then to consist in magnetizing the point of contact, which was accomplished as follows. Figs. 2 and 3 rep-

resent different views of a small car which in figure 4 rests on a railroad. The driving wheels of this car are surrounded in their lower part by a helix H, containing a conducting wire wound a certain number of times on itself, and constituting thus a true

4.



magnetizing helix. It is fixed, and embraces the lower part of the wheel; its inner sides are far enough apart for the wheel to turn freely without touching the helix. On passing the electric current, the wheel feels the two fluids, one, the boreal, for ex-

ample, influencing the part of the wheel above the helix, the other, austral, the lower portion; and as the helix is placed as near as possible to the point of contact, this last portion must be more strongly magnetized than the upper, the austral being concentrated on the smaller space.

The apparatus in figure 4 served to verify these different effects. The roller T is for receiving the cord which is wound upon the motor axle E (fig. 3), and which corresponds to the moving power; P represents the train to be transported.

The weight P is increased until the car is ready to slide back; as soon as the motion ceases, the current is let on, and immediately the car starts off and draws the weight P; it descends again if the circuit is broken, and reascends on establishing it again, and so on.

In this apparatus, each of the helices H and H′ consists of a brass box containing each 8 meters of copper wire of $0^{m}{\cdot}0071$ in diameter, distributed in 77 turns, or 16 meters and 154 turns for both. The cord which sustains the weight P is fixed on N (fig. 3) and passes over the pulley behind; G represents a voltaic couple; C a railroad track, arranged so that the grade may be varied.

The following trial gives nearly the relations existing between the two modes of adhesion operating in this apparatus and between the pressures which produce the adhesion.

Weight of car 5 kilogrammes. Grade 190 millimeters to the meter. One element of a battery of gas carbon. Total current tan 35° 10′. Reduced current of the helices, tan 18° 25′.

Weight required for motion.		Sum of adhesions.	Additional weight required to replace the magnetic adhesion.
Ordinary adhesion.	Magnetic adhesion.		
5·500 kil.	5·100 kil.	10·600	3·500

The two driving wheels were fixed to the rails with a magnetic force equivalent to $3\frac{1}{2}$ kilogrammes;—this force was measured by means of a dynamometer, with which the car was detached in a direction perpendicular to the plane of the track.

This experiment clearly proves, that between the magnetic pressure and its adhesion, there is the same relation as between the ordinary adhesion and the pressure by added weight. From another experiment we have also learned that the same relation subsists between the attraction exerted by an electro-magnet, on a surface of iron, and the weight required to make it move. The magnetic co-efficient for moving equals closely, the co-efficient afforded by a mass of iron whose weight was equal to the magnetic pressure exerted by the electro-magnet.

In the apparatus of figure 4, there is also on each side an electro-magnet F, fixed to the grease boxes, and suspended by means of springs, *r*. These electro-magnets act as a check; unlike the checks in use on railroads, they act upon the rails, and thus preserve the wheels which are rapidly worn by the ordinary checks;

there is nothing to prevent making the magnetic checks to act upon the wheels. In either case, a power as energetic as desired may be developed, whose intensity may be varied at will, from that required to produce an instantaneous arrest of the motion, as in cases of imminent danger, to that for ordinary stoppages.

These different effects were produced with the small car represented in figure 4. The element of a galvanic battery here employed, gives a force sufficient to stop the car instantly, even when driven at full speed it arrives at the bottom of a grade of 190 millimeters. By diminishing the current or augmenting the charge, we produce on the contrary an effect which retards the motion of the vehicle and ends by stopping it altogether. By interrupting the current and reëstablishing it in turns the car may be made to leap along the track. These different effects, which are not well attempted with a large train, are readily shown on a small scale, and are calculated to interest much the young student.

But it will be understood that my researches aim not merely to exhibit a new property of electro-magnets, and combine them in instructive apparatus. My plans are of another kind. They seek to contribute to railroads, a new and important improvement, and I shall not be satisfied until it has become easy to use grades of more than 10 millimeters to the meter, and until it shall become no longer necessary to construct tunnels at great expense or to build extensive earth-works, or make curves of large radius. Of what avail otherwise the efforts to give a smaller size to the driving wheels, less weight to locomotives, lighter rails to the road, thereby to make a large diminution of expense in the construction and use of roads? If a smaller and lighter driving wheel be invented, the adhesion must be imparted to it which it fails of having on account of its lightness, and hence it must be useless under the old system which works upon the paradox of making a machine run better by giving it more weight.

I have therefore sought to pass from the laboratory experiment to the actual locomotive in its own field. I have made my trials on the road between Paris and Lyons. The locomotive put at my disposal was, in truth, somewhat worn by long service, and one of its driving wheels carried a ton of weight more than the other. I was not allowed to make any changes in the machine, and I was required to attach my apparatus in such a way that it might be easily removed. Notwithstanding the unfavorableness of these conditions I went to work, certain of having an effect sufficient to prove the application possible. This was admitted by a commission appointed by the minister of Public Works, consisting of MM. Pouillet, Regnault, Froment, Le Chatelier, Chatelus and Sauvage, chief engineer of the Lyons railroad. The useful effect observed in this experiment was about 9 per cent.

Under the new conditions, obstacles were encountered which were unknown in my laboratory researches. Still, without modifying the wheels, which was not allowed, I undertook to apply the method of magnetization above described.

As the experiments have led to some curious results not devoid of scientific interest, I will enter into some details which may at least serve for verification, and will excite the attention of physicists engaged in this department of electro-magnetism. The points which will especially engage my attention in this essay will be the battery, and particularly the helices. The helices when once constructed must remain as they are, while, by the different modes of combination of which a battery is susceptible, we can always apportion more or less the tension of the fluid to the resistance to be overcome.

Helices of Magnetization.

Considering only the works hitherto published on magnetization, the nature of helices should be easily resolved. In fact, according to Dal Negro, Fechner, Lenz and Jacobi,*

1. *The magnetic intensity developed is proportioned to the intensity of the current.* The nature of the application is opposed, it is true, to a galvanic development of great power; but it may be supplied, if, as Lenz and Jacobi conclude,

2. *The attraction is proportioned to the intensity of the current multiplied by the number of turns in the helix;* and with much more reason, if as says M. Dub,†

3. *The magnetic attraction developed, is proportional to the square of the current multiplied by the square of the number of turns of the spire;* a law which ten years since Mr. Joule ‡ expressed in the formula, $M = E^2 W^2$, M, representing the magnetism developed, E, the quantity of electricity in activity; and W, the length of the conductor.

Moreover, MM. Lenz and Jacobi, say that "*the section of the conducting wire is without influence on the magnetizing force, provided the galvanic intensity does not vary;*" and as the resistance offered by the conductors is proportional to their length and inversely as their section, a battery and helix may be constructed such that the number of turns of the helix shall be the greatest possible. Happily I have had occasion to observe that Mr. Joule's formula is not exact, or at least does not apply in such a case as this; the law of magnetic maximum of M. Müller is on the other side opposed to it.

These doubts and contradictions, and the necessity of further experiment, complicates much the problem in hand. I have how-

* Poggendorff's Ann., xlvii, 225. † Poggendorff's Ann., (7) lxxxi, 46.
‡ Phil. Mag., [4] vii, 309, and Annals of Elect., i, 470.

ever, succeeded in meeting the several points, and in making the resistance of the battery equal with that of the conducting wire, without too considerable a development in either the battery or helix. The wire was $4^{mm}\cdot5$ in section, with a total length of 1036 meters; each helix received 518, forming 216 turns.

The battery consisted of 64 elements of Bunsen made with gas carbon; the surface of the carbon in each element was $0^{m}\cdot911$; that of the zinc immersed $0^{m}\cdot9148$. These 64 elements were arranged in 8 cases, having each compartments covered with gutta percha, as has been done by Prof. Page. The whole was placed behind the tender. In this position the battery was out of the way, and hardly seen in the train.

This battery whether arranged for tension, or coupled for quantity in series of 32 elements with double surface, produces the same results whenever an equivalent direction is given to the current. Thus when arranged for tension, a single current is produced; and when for quantity, the current is diverted on arriving below the machine, so as to form a distinct current in each helix; two conductors of copper wire answer in this experiment which is not quite delicate in other respects on account of the excessive conductibility of the materials which enter into the construction of a locomotive. These conductors were lodged in tubes of gutta percha, which were themselves covered with chamois leather, that was impregnated with varnish especially where these tubes are directly supported by iron. On reaching the point of junction of the tender with the machine, these conductors are adapted to the conductors fixed to the locomotive, being bifurcated with them in a way to avoid rupture from the vibrations of the cars while in motion. From thence, they follow the left side of the locomotive, penetrate under the machine between the furnace and one of the driving wheels, and are then continued parallel to the machine, half way to the helices. Being firmly fixed to the points of attachment presented in great number under the machine, these conductors are denuded where they receive the extremities of the wires proceeding from the helices. These last were suspended to the grease-boxes by means of strong clamps and a plate of iron.

Thus arranged, one of the conductors is continuous from one extremity to the other; the other conductor on the contrary, has a break produced by the commutator. On arriving near the iron seat ("coffre") on the front of the tender, breast-high, this conductor diverges from the line which it has before followed, and is secured to the extremity of a plate of platinised copper, attached solidly to the seat, and carefully insulated. The other side of this connecting plate is destined to receive the extremity of the interrupted conductor which is in contact with one of the poles of the battery.

This part of the conductor is flattened, so as to present a convenient surface; a spring holds it off from the plate, and in this state the circuit is open; to close it, a key is touched which acts on the conductor, and presses it upon the connecting plate; and it is broken by a contrary movement, when the spring throws off the conductor.

On making the battery connection, a magnetic radiation is perceived to a great distance, so that at 5 meters from the wheel in the plane of the helix, small strips of iron may be magnetized.

The result of the experiment made with the apparatus just described, has been made the subject of a report to the Minister of Public Works, by the commission already named. The following numbers are cited from this document.

The trial was made on a grade of 10 millimeters to the meter. The elements of the machine, were as follows;

	Meters.
Diameter of pistons, - -	0·400
Play of the pistons, - -	0·600
Diameter of the driving wheels,	1·600
Heating surface of the fire, -	779·600
" " " tubes, -	7·860

Weight of the machine provided with its water and coke, - - - - - - 29 tons.
Load on the rails to be drawn by the driving wheels, 14 tons.
Pressure of the boiler, - - - - 6 atmospheres.
The whole train, about - - - 119 tons.
The rate of motion, per hour, - - 15 kilometers.

Under a pressure of 7 to 8 atmospheres, the locomotive slid easily over the rails when the galvanic circuit was open; the movement ceased when it was closed, and began again when again open. To make the locomotive move in spite of the magnetism, a pressure of 9 atmospheres was required with a risk of bursting the boiler which was registered for six atmospheres. Under this pressure, the driving wheels moved in both cases, yet with an apparent difference when the wheels were magnetized.

The additional adhesion from magnetism was thus proved, although it was less than could have been obtained with a more convenient arrangement. In fact the particular process followed in this trial, did not retain the magnetization quite constant at the point of contact of the wheels; this magnetization diminished with the rotation, as shown in the following table, which gives the results of trials with a single wheel, from rest to a velocity of 18 kilometers an hour.

Turns per minute.	Kil. per hour.	Pressure due to Magnetization.	Relation of *additional* pressure to the *ordinary* pressure on the rail.
0	0	639 kil.	12·8 to 10·6 p. c.
30	9	255	5·1 to 4·3 p. c.
60	18	165	3·3 to 2·7 p. c.

In this experiment the wheels did not rest on the rail as in ordinary conditions: by means of the grease-box reversed of each, they were fixed upon a framework for their support; two wooden pulleys were arranged on the motor axle; these pulleys, in connection with the power wheel of the work shop, were calculated so as to furnish the two velocities of rotation, registered in the table. The rail placed below each wheel was moveable at one of its extremities, around a bolt, and carried at the other, a box of zinc for receiving the weight necessary for detaching the rail from the wheel when it was suspended by the magnetising action; a reservoir of water, divided into litres, was arranged so as to turn into each box the water necessary for breaking the connection.

Kowing the length of the rail, its weight, the same of the box of zinc, and the quantity of water required from the reservoir, we may, by a simple calculation, ascertain the amount sustained by the point of contact, and thence the pressure due to the magnetisation, either during rest or rotation.

In this way the table above has been obtained. The decrease observed during rotation was also exhibited in the following manner. The rail resting at its free extremity on a block of wood, and being separated from the wheel by an interval of 0·17 metre, a small magnetic declination needle placed at a distance of 0·75 metre from the rail, was moved along the the wheel in order to ascertain the points where its direction was perpendicular to the plane of the wheel.

When the wheel was at rest, the needle took a position vertical to the rails only when in a vertical plane passing by the axle, and consequently at the point of contact with the wheel. When on the contrary, the wheel was in motion, it was necessary to change the place of the needle, moving it backward (in relation to the direction of the movement of the wheel) in order to obtain the direction normal to the rails. This change of place amounted to 0·33 and 0·39 m. for velocities corresponding to rates of 18 and 36 kilometers to the hour.

These observations show that the resultant of the magnetic forces which passes by the centre of gravity of the helix, and consequently by the point of contact of the wheel when it is in repose, becomes displaced during the movement, and is no longer identical with the point of contact. This arises from two distinct causes. At the moment of entering the helix, the portion of the rim of the wheel under consideration experiences a first effect of magnetisation tending to impress upon it a polarity conformed to that part of the helix which is opposite to the point of contact. Reaching this point, it should be at its maximum power; but in presence of the contrary fluid of the superior part of the helix, its magnetism diminishes while it is rising and moving from the helix, and the fluid has changed its name when the rim has arrived at the

emergence. These contrary effects of magnetisation and demagnetisation, and subsequently of inversion of the fluid, might be made of use, if contrary to the fact, the rim was made of soft iron, having no coercitive force. Hence, at each revolution, the wheel is subject to double magnetic work, which is unequal, and must give to the resultant of the magnetic actions, a position of equilibrium different from that existing when the wheel is at rest.

At rest, the wheel does not differ from an ordinary electro-magnet; like it, it is divided into two parts magnetically distinct; the turns of its helix are parallel to the plane of the armature; the resultant of the magnetic actions developed by the helix is perpendicular to this plane, and passes by the centre of gravity of the helix. The position of the pole is then subordinate to that of the helix. If the former is displaced 0·33 meter it is only necessary to move the helix forward a corresponding quantity to remedy the displacement of the pole, which will be easily managed, if no other improvement is required.

One of the most important improvements consists in the possibility of bringing into simultaneous action the two magnetic poles, to profit by the increase of force that always takes place when both poles act together on an armature. The trial reported by me in this Journal,* evinces the importance of this condition.

The improvements which I have explained as regards the general principle, in the July number of this Journal, in my article on circular electro-magnets, aim principally towards,

1. Preventing the displacement of the poles.
2. Giving constancy to the magnetisation of the wheel.
3. Bringing both poles into concurrent action.

The concurrent action of both poles is made certain by the check described above, as well as by the more convenient electro-magnet which I have made known in the January number of this Journal, and which may be also of use as a check on railroads.

The researches which I have here described, constitute the first part of my experiments. It would have been difficult to have carried them thus far, if I had not found in one of the most distinguished engineers of France, a Mæcenas who appreciated at first sight the importance of my labors, and by a rare generosity enabled me to undertake these researches. M. Bazaine, in whose life many facts of the kind might be mentioned, needed not this new act of disinterestedness, that his love of progress should be known. He was among those who introduced railroads into France, and he gave the country its first great road, that from Strasbourg to Basle.

* Vol. xv, p. 104, Jan., 1853, and p. 380, May.

ART. XXXIV.—*On the Passivity of Nickel and Cobalt;* by M. J. NICKLÈS.

THE singular property of iron of becoming less oxydisable in contact with fuming nitric acid, has engaged the attention of many chemists and physicists. Since its discovery by Keir, it has been examined from different points of view by Herschell, Faraday, Schönbein, Buff, de la Rive, Andrews, Mousson, Millon, Beetz, and Bollmann. The researches of these investigators have shown that iron may become passive not only in contact with fuming nitric acid; it acquires this property also on blueing it in the flame of a lamp, or on touching it with a plate of platinum, while it is plunged in nitric acid not fuming.

The same effect is produced when the iron is put in connection with the positive pole of a galvanic battery. As evidence of the modification which it undergoes under these circumstances, the iron does not precipitate sulphate of copper when it is used as the anode of a galvanic element; oxygen is disengaged around it, without attaching it; in contact with diluted nitric acid, it remains unaltered, but it becomes again active when after being taken from the acid, it is put into water.

Similar facts may be observed to a greater or less extent with nickel and cobalt drawn into wire. The wire of these metals used in the experiments was chemically pure; it was prepared by M. H. Sainte Claire Deville, by a process of heating of his invention,* who also analyzed it.

In fuming nitric acid, both of these metals acquire a passive state only of short duration; but the passivity becomes permanent when, after blueing them in the flame of an alcohol lamp or on a charcoal fire, they are plunged while hot into this acid, in which case, they act in every respect like passive iron, except that they are less negative than in nitric acid. They can however, communicate their passive state to active iron plunged in nitric acid not fuming, and so arrest the energetic action that is produced by the acid.

Platinum is always negative with respect to these three metals in the passive state, and either of these last is negative with respect to the same in the active state.

The preservation of iron against attack in sulphate of copper, observed by M. Schönbein, has not been obtained out of the galvanic current; in all my experiments, the passive iron becomes promptly covered by metallic iron.

I have also examined the electro-chemical relations of iron, nickel and cobalt in the active and passive states, in contact with different acids as well as a solution of potash in water. The negative character of passive iron is really very decided only in

* See this Journal, xv, 424.

nitric acid; in the other liquids used in the trials, positive electricity passes to the iron instead of proceeding from it. In the potash liquid, the reactions have been the same both for the active and passive metals, which seems to show that the passivity of these related metals is destroyed by this alkali; in fact, if after this immersion in potash, they are put in contact with nitric acid of density 1·34, they are found to have resumed the electro-chemical condition which belongs to the passive state and they are no longer attacked by the acid.

The following series exhibits the relations respectively of iron, nickel, and cobalt to the two states, commencing with the positive metal, and ending with the negative; the experiments were made with the different liquids mentioned in the table:

Liquids employed.	Active Metals.	Passive Metals.
Fuming nitric acid.	+ . . . − .	+ Co, Ni, Fe, −
" " G= 1·34.	Fe, Co, Ni.	Co, Ni, Fe.
$\overline{S}$ $\dot{H}$	Co, Fe, Ni.	Ni, Co, Fe.
$\overline{S}$ $\dot{H}$, with 9$\dot{H}$.	Fe, Ni, Co,	Fe, Co, Ni.
Potash solution.	Fe, Ni, Co.	Fe, Ni, Co.

Palladium is equally susceptible to the passive state.

ART. XXXV.—*Method of taking Daguerreotype Pictures for the Stereoscope, simultaneously, upon the same plate, with an ordinary Camera;* by F. A. P. BARNARD, Professor of Chemistry and Natural History, in the University of Alabama.

Prof. DANA.—In the September number of the Journal of Science, just published, I observe a mention of a method of taking photographic pictures for the stereoscope, the two pictures being taken simultaneously. This has brought to my mind an arrangement employed by myself about a year ago for a similar purpose, which is so simple and satisfactory in its results, that you may possibly think it worth publishing. It has the advantage of requiring no modification of the construction of the camera; and also the additional one of producing both pictures, if desired, upon one plate. This last result cannot be secured with a camera having two object-glasses, (without, at least, a very inconvenient arrangement of mirrors,) because, of the two pictures produced in such a camera upon one plate, the right hand one will be that which should belong to the left eye, and vice versa.

Fig. 1 is a plan of the arrangement which I have employed. C is the camera, P a central point in an object to be copied, and AM, AM, two small vertical plane

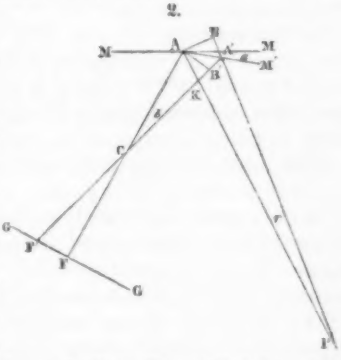
1.

mirrors, moveable on a common vertical hinge at A. These mirrors are at first brought truly to the same plane, so that they give but one image of the object on the ground glass screen of the camera, at F. The camera is then adjusted so that, the image of P being single, the optical axis FA may be directed truly toward the hinge A, and the image be formed truly in the middle of the screen, at F. Now supposing that it is desired to produce two pictures distant from each other (measuring from centre to centre) by a space $=n$, the two mirrors must be carefully moved on the hinge A to the positions AM′ and AM″, so that the images of P reflected by them shall pass from F to f, and from F to f', each of these distances being $\frac{1}{2}n$.

In order that the points of view under which these images will present P, may be so far different as to correspond to those of the two eyes in natural vision, the camera must be placed at a certain determinate distance from the mirrors. This will be easily ascertained without calculation by a person familiar with this process; but it may be found mathematically as follows:

Let AM, AM, fig. 2, be the two mirrors, and A the hinge. Then, the camera being supposed to be properly adjusted, AF will be the line of its axis, and also the direction of the ray PA after reflection, while the mirrors continue in one plane. Let AM′ be the position of one of the mirrors after its displacement. Then if C be the virtual centre of the arrangement of lenses, the image of P will be formed at F′ instead of at F, by means of the ray PA′ reflected through C to F′. GG′ the glass screen, will of course be perpendicular to the axis AF.

2.



Draw AB perpendicular to AP, and AB′ perpendicular to AF. Put the angular change of position of the mirror M′ (=angle MAM′) $=\alpha$, the angle ACA′ $=\delta$, and the angle APA′ $=\gamma$. Then in the triangle PAB, right-angled at A, angle B $=90°-\gamma$. It is easily seen that BAM = the original angle of incidence of PA. Represent this angle by I. Then,

$$\text{BAM}+\text{MAA}'=\text{BA}'=\text{I}+\alpha$$

Also, as above, $\text{ABA}'=90°-\gamma$

Whence, in the triangle BAA′, the third angle, $\text{BA}'\text{A}=90°-\text{I}-\alpha+\gamma$.

Now, to obtain AA′ in terms of AB,

$$\sin \text{BA}'\text{A} : \sin \text{ABA}' :: \text{AB} : \text{AA}',$$

Or, putting $AB = a$

$$\sin(90^\circ - I - \alpha + \gamma) : \sin(90^\circ - \gamma) :: a : \frac{a\cos\gamma}{\cos(I + \alpha - \gamma)}.$$

Again, in the triangle $B'AA'$,

Angle $AB'A' = 90^\circ + \theta$.

And, $AA'B'$ $(= AA'B) = 90^\circ - I - \alpha + \gamma$.

Whence $\sin AB'A' : \sin AA'B' :: AA' : AB'$.

Or $$\sin(90^\circ + \theta) : \sin(90^\circ - I - \alpha + \gamma) : \frac{a\cos\gamma}{\cos(I + \alpha - \gamma)} : AB'$$

And $$AB' = \frac{a\cos\gamma\cos(I + \alpha - \gamma)}{\cos\theta\cos(I + \alpha - \gamma)} = \frac{a\cos\gamma}{\cos\theta}.$$

Now AB' is parallel to GG : hence,

$$FF' : AB' :: FC : CA,$$

which last term is the distance (measured from the virtual centre of the objective) at which the camera must be placed from the point A.

In this proportion, FF' is arbitrarily fixed, and will be from 1 to 1¼ inches, FC is the focal distance of the camera, when the image of P is distinct on the screen, and AB' is determinable by the foregoing formula.

In that formula, α is one half the distance between the eyes (1¼ inches on an average), θ is directly determinable in the right angled triangle CFF', and γ is in like manner to be obtained from the right angled triangle PAB, the distance, AP, of the object from A, being ascertained by measurement.

The mirrors ought to be such as are prepared for photographic purposes;—that is to say, they should be of the best glass, and have their surfaces perfectly parallel, or else they should be of metal. Nevertheless I have succeeded very well with good plate looking-glass. I send you a specimen taken with such, that you may compare it with specimens otherwise prepared. The imperfections of the glass will be less sensible, in proportion as the incidence approaches perpendicularly; but there is an obvious limit to this.

The photographs which I have prepared in this way are not surpassed by any others I have tried. I am accustomed to adjust them on the plate at a distance from each other somewhat less than that of the eyes (say between 2 and 2¼ inches from centre to centre). I employ no optical artifice, (i. e. interposed prisms, or lenses excentric to the eyes,) to superpose them; but looking through the centres of the lenses, the superposition takes place naturally and easily. If the pictures are rather large, however, they must be more widely separated, and then some optical expedient must be employed to produce deflection and aid the eye.

Tuscaloosa, Sept. 6, 1853.

P. S.—In every daguerreotype for the stereoscope which I have seen (as purchased from the opticians) the relief is grossly exaggerated. You will not find such the case with this. The error of the manufacturers has been to make the points of view (in taking the photographs) too widely different.

ART. XXXVI.—*Theoretic Determination of the Expenditure of Heat in the Hot-air Engine: Supplementary Article;* by FREDERICK A. P. BARNARD, Professor of Chemistry and Natural History, in the University of Alabama.

A NEAT and direct expression may be obtained, for the total consumption of heat in the air engine, in terms of the numerical ratio between the cylinders, the fractional part of the stroke of the piston before cut-off, and constants. We must include in this, however, a loss from a source not considered in the article published in the last number of this Journal; viz., that which is occasioned by the expansion of the air at the moment of final escape. It is evident that if, at the close of the stroke, the air in the working cylinder has a higher elasticity than that of the atmosphere, it will expand as it emerges, until an equilibrium is established, and a portion of its heat will disappear.* This is provided for in the following. We will employ, as before,

T, to represent the working temperature.
θ " " that of the weather.
θ' " " that of the air after compression.
θ'' " " that of the same at the moment of escape.
Θ " " the reciprocal of the coëfficient of expansion, (i. e. 491° F.)

The other symbols employed in the former article, will be similarly employed here.

It has been shown that, of the heat received from the furnace, there is directly converted into expansive power an amount equal to $(T-\theta')(\gamma-1)$. There is also lost by expansion, in the act of escaping, the additional amount of $(T-\theta'')$. And there is a compensation, in consequence of cooling under the constant atmospheric pressure, equal to $(\theta''-\theta')(\gamma-1)$. These expressions united, are reducible to

$$(T-\theta'')\gamma,$$

which is a general expression for the total expenditure.

But θ'' is not a constant. While T and θ remain the same, it will be a function of l and m, the length of the stroke before cut-off, and the ratio of the cylinders to each other in cross section. By Poisson's first equation,

$$p'=p\left(\frac{\varrho'}{\varrho}\right)^{\gamma},$$

* To the truth, in every particular, of the paradox introduced by way of illustration, in the commencement of the former article, it is evidently necessary that the air which is supposed to escape after being heated without previous compression, should be prevented from expanding as it escapes;—as, for instance, by being returned directly to the supply cylinder. Practically, however, it would be impossible for the regenerator to abstract from this air all the heat it had received; but if it could, the result would be as there stated.

in which p' is the working pressure, ϱ' the density of the compressed air, p the atmospheric pressure, and ϱ the density of the atmosphere, assumed $=1$. Now, if ϱ'' represent the density of the air in the working cylinder at the moment of cut-off, and ϱ''' the density at that of final escape, we shall have, by virtue of the same equation, for the *pressure at escape* (represented by p'')

$$p''=p'\left(\frac{\varrho'''}{\varrho''}\right)^{\gamma}.$$

In this, if we substitute the foregoing value of p', we obtain

$$p''=p\left(\frac{\varrho'}{\varrho}\right)^{\gamma}\left(\frac{\varrho'''}{\varrho''}\right)^{\gamma}$$

But, as in the nature of things, p'' must equal p, this equation becomes,

$$1=\left(\frac{\varrho'}{\varrho}\right)^{\gamma}\left(\frac{\varrho'''}{\varrho''}\right)^{\gamma},\text{ or }\left(\frac{\varrho}{\varrho'}\right)^{\gamma}=\left(\frac{\varrho'''}{\varrho''}\right)^{\gamma}.$$

Now, by Poisson's second equation,

$$\theta''=(\Theta+\mathrm{T})\left(\frac{\varrho'''}{\varrho''}\right)^{\gamma-1}-\Theta.$$

Whence, also,

$$\theta''=(\Theta+\mathrm{T})\left(\frac{\varrho}{\varrho'}\right)^{\gamma-1}-\Theta,$$

And we have no occasion to determine ϱ''' and ϱ''.

For ϱ' we proceed thus. In the last number of the Journal we have these two expressions.

$$n=\left(\frac{\mathrm{T}+\Theta}{\Theta+\theta}\right)^{\frac{1}{\gamma}}\left(\frac{l}{m}\right)^{\frac{\gamma-1}{\gamma}},\text{ and }\varrho'=\frac{mn}{l},\text{ or }\frac{1}{\varrho'}\left(=\frac{\varrho}{\varrho'}\right)=\frac{l}{mn}.$$

Substituting the value of n in the last expression, we obtain, after reduction,

$$\frac{\varrho}{\varrho'}=\left(\frac{l}{m}\left(\frac{\Theta+\theta}{\Theta+\mathrm{T}}\right)\right)^{\frac{1}{\gamma}}.$$

And by substituting again in the preceding formula, we have finally,

$$\theta''=(\Theta+\mathrm{T})\left(\frac{l}{m}\left(\frac{\Theta+\theta}{\Theta+\mathrm{T}}\right)\right)^{\frac{\gamma-1}{\gamma}}-\Theta$$

$$\text{Or, }\quad\theta''=\left[\left(\frac{l}{m}(\Theta+\theta)\right)^{\gamma-1}\left(\Theta+\mathrm{T}\right)\right]^{\frac{1}{\gamma}}-\Theta.$$

As the factors $(\Theta+\theta)^{\frac{\gamma-1}{\gamma}}$, and $(\Theta+\mathrm{T})^{\frac{1}{\gamma}}$, may be regarded as constant, we may represent their product by a single symbol, as N, which being substituted will simplify the expression thus:

$$\theta''=\mathrm{N}\left(\frac{l}{m}\right)^{\frac{\gamma-1}{\gamma}}-\Theta.$$

Whence for the total consumption,

$$(T-\theta'')\gamma=(T+\Theta-N\left(\frac{l}{m}\right)^{\frac{\gamma-1}{\gamma}})\gamma.$$

Since we have all along regarded T as equal to 450°, Θ to 491°, and θ to 60°, $T+\theta$ will be =941°, and N=791°·663. Hence finally, while these suppositions remain,

$$(T-\theta'')\gamma=(941°-791°\cdot633\left(\frac{l}{m}\right)^{\frac{\gamma-1}{\gamma}})\gamma.$$

From this formula, making $\gamma=1\cdot36$, as before, we shall have, for the case of equal supply and working cylinders,

$$186\cdot55\,MK_v=137\cdot17\,MK_p=32\cdot59\,MK_w,$$

which is a larger result than was obtained in the former article, while the effect of final expansion was disregarded. Supposing, as before, that the cylinder will contain 70 lbs. of air, the entire consumption of heat would be sufficient to raise a pound of water $70\times32°\cdot59=2281°$, or to convert nearly two pounds of water into steam at 212°. The power of this steam, computed as in the former article, would be sufficient to raise 115000 lbs. one foot, or 19100 through the six foot stroke. But, as we have seen that the air engine will raise, on the present supposition, 56400 lbs. six feet with the same expenditure, the expansion is in favor of air in the ratio 1 : 2·95.

Supposing the cut-off at $\frac{2}{3}$, the mean pressure will rise to 116600 lbs., being an increase of more than 100 per cent., while the expenditure of heat will be increased but about 60 per cent. The steam which this heat would generate, would raise 30500 lbs. through the six-foot stroke, showing an advantage in favor of air, as 1 : 3·80. But if the steam is allowed to expand to the same extent as the air, the advantage will be reduced to the ratio, 1 : 2·80.

A supply cylinder equal to the working cylinder, however, is too large for economy. This is a consequence of the loss of heat by expansion, in escaping, spoken of in the commencement of this article; and it could not be true if the regenerators were capable of arresting the heat remaining unexpended at the close of the stroke, to the extent anticipated by the inventor. Taking the proportions of Ericsson's cylinders, we shall have, for a $\frac{2}{3}$ cut-off, 186 units of heat consumed to every unit-weight or pound of air. But as the air is now but $\frac{2}{3}$ of what was before supposed, our multiplier will now be 47 instead of 70. On completing the calculation, we find that heat enough will be expended, to raise, by means of steam at 212°, 12830 lbs. six feet. But the air engine, in this case, will raise 77660 lbs.,* to the same height with the same expenditure of heat; so that, here, the advantage is in favor

* Stated incorrectly seventy-*four* thousand pounds, in the former article.

of air in the ratio, 1 : 6·05. If again, we allow an equal expansion to steam, the advantage will be reduced to the ratio 1 : 4·40.

In all these computations, as in the former article, we have considered the value of the *co-efficient of capacity* for air (or γ) to be only 1·36. But there can now be no doubt that this ought to be put =1·40, or 1·41. Mr. Rankine (Lond. and Ed. Phil. Mag., June, 1853) adopts 1·4094, which is employed in the computations that follow. The effect of the enlargement of this number, is to diminish somewhat the calculated power of the air engine, in every case.

For the sake of making apparent, in a single view, the effect of variations in the proportions of the cylinders, and of different positions of the cut-off, the following table has been prepared, exhibiting a large variety of suppositions. The little probability which seems now to exist, that any further attempt will be made to apply the invention in practice, renders the calculations rather curious perhaps than useful; but they may serve to throw some light upon certain questions which have been topics of discussion with different writers on this subject.

In this table, the symbols l and m stand as in the former article, for the length of the stroke before the cut-off (the entire stroke being unity), and the numerical ratio in cross section between the cylinders (that of the working cylinder being unity). In the column headed "heat expended," are given the numbers of units (or degrees F.) which the total theoretic expenditure would be capable of imparting to one unit of weight (or pound) of water. The column headed R, shows the theoretic ratio of advantage in favor of air, as compared with the steam which the heat expended would produce at 212°, the steam being supposed to work without expansion. The column R′, shows a similar ratio, on supposition that the steam expands as much as the air. Since these expansions are very variable, the numbers in this column are a less eligible criterion by which to judge of the relative theoretic economy of air and steam engines, than those of the column R. But as steam is often worked with much larger expansion than any that air admits of, an additional column is introduced, headed R″, which shows the effect of heated air as compared with that of the steam generated by the same heat, and expanded to three times its original bulk.

All the powers in this table are computed by means of the formula, p. 248, of the last number of this Journal,* and on the sup-

* In this formula as there printed there occurs an error. In the last term within parenthesis, the factor $l^{\gamma-1}$ should be $l^{\frac{\gamma-1}{\gamma}}$.

Also, in the particular formula, top of the following page, the numerator of the first fractional term should read $\lambda\mu^{\gamma}(\lambda^{\gamma-1}-1)$ instead of $\mu^{\gamma}(\lambda^{\gamma}-1)$. These errors are discoverable by following out the steps of the processes which precede them;

position that the surface of the working piston is 22100 square inches, the stroke six feet, and the number of revolutions, ten.

Aggregate horse-power of air engines, with the corresponding expenditure of heat, and a comparison with steam.

m	l	H. P.	Heat expended.	R.	R′.	R″.
1	1	197	2486°	1 : 2·60	1 : 2·60	1 : 1·30
·85	·85	279	2113	1 : 4·33	1 : 3·73	1 : 2·16
·80	·80	290	1989	1 : 4·65	1 : 3·84	1 : 2·32
·7646	·7646*	292	1900	1 : 5·05	1 : 4·00	1 : 2·52
·75	·75	291	1864	1 : 5·23	1 : 4·12	1 : 2·61
·70	·70	284	1740	1 : 5·53	1 : 3·99	1 : 2·76
$\frac{3}{4}$	$\frac{3}{4}$	273	1657	1 : 5·41	1 : 3·95	1 : 2·70
·65	·65	268	1616	1 : 5·40	1 : 3·87	1 : 2·70
·60	·60	241	1492	1 : 5·31	1 : 3·63	1 : 2·65
1	·85	303	3094	1 : 3·20	1 : 2·77	1 : 1·60
1	·80	333	3312	1 : 3·29	1 : 2·72	1 : 1·64
1	·75	360	3542	1 : 3·34	1 : 2·52	1 : 1·67
1	·70	384	3783	1 : 3·32	1 : 2·50	1 : 1·66
1	$\frac{3}{4}$	398	3949	1 : 3·29	1 : 2·49	1 : 1·64
1	·65	403	4036	1 : 3·26	1 : 2·33	1 : 1·63
1	·60	415	4302	1 : 3·25	1 : 2·23	1 : 1·62
·85	·80	·308	2309	1 : 4·37	1 : 3·61	1 : 2·18
·85	·75	336	2512	1 : 4·38	1 : 3·45	1 : 2·19
·85	·70	359	2727	1 : 4·31	1 : 3·24	1 : 2·15
·85	$\frac{3}{4}$	374	2875	1 : 4·26	1 : 3·11	1 : 2·13
·85	65	381	2932	1 : 4·14	1 : 2·99	1 : 2·07
·85	·60	397	3190	1 : 4·08	1 : 2·79	1 : 2·04
·80	·75	319	2183	1 : 4·73	1 : 3·75	1 : 2·36
·80	·70	340	2390	1 : 4·68	1 : 3·52	1 : 2·34
·80	$\frac{3}{4}$	356	2530	1 : 4·61	1 : 3·36	1 : 2·30
·80	·65	362	2606	1 : 4·59	1 : 3·27	1 : 2·29
·80	·60	378	2833	1 : 4·37	1 : 2·19	1 : 2·18
·75	·70	315	2047	1 : 5·00	1 : 3·77	1 : 2·50
·75	$\frac{3}{4}$	331	2197	1 : 4·90	1 : 3·59	1 : 2·45
·75	·65	340	2267	1 : 4·88	1 : 3·50	1 : 2·44
·75	·60	355	2484	1 : 4·67	1 : 3·20	1 : 2·33
·70	$\frac{3}{4}$	299	1870	1 : 5·23	1 : 3·80	1 : 2·61
·70	·65	306	1936	1 : 5·17	1 : 3·67	1 : 2·58
·70	·60	328	2144	1 : 5·01	1 : 3·43	1 : 2·50
$\frac{3}{4}$	·75	235	1349	1 : 5·67	1 : 4·61	1 : 2·83
$\frac{3}{4}$	·70	259	1532	1 : 5·53	1 : 4·15	1 : 2·76
$\frac{3}{4}$	·65	281	1722	1 : 5·32	1 : 3·84	1 : 2·66
$\frac{3}{4}$	·60	301	1922	1 : 5·14	1 : 3·52	1 : 2·57
·65	·60	287	1813	1 : 5·18	1 : 3·55	1 : 2·59

The column R″ shows that there is still a large theoretic balance in favor of air, as respects economy of heat, even when steam is worked with pretty large expansion. Moreover, the ad-

but as these processes are not all simple, the misprints may not be detected without notice of their existence.

In the numerical value of the maximum just below, ·71 should be ·76.

There are a few errors in the text which somewhat obscure the meaning. In line 7, p. 235, "undeniable" should be undesirable. In p. 225, line 22, "properties" should be proportions. In p. 245, last line but one, "fraction" should be function.

* In the first division of the table, l is made equal to m for various values. In this case the maximum power is obtained with $l = m = ·7646$.

vantage is somewhat greater than it appears in the table, since this column is computed by regarding the expansion of steam as conforming to the law of Mariotte.

There are large drawbacks, however, which have not been considered. In the first place, the regenerators cannot possibly (as our supposition has presumed) abstract from the escaping air all the heat down to the temperature θ'. There must also be great loss by radiation and conduction. Much heat is carried off by the leakage of the heated air, and finally the escape of heat through the smoke pipes cannot but be largely greater than occurs in heating water in boilers.

Capt. Ericsson allows a loss of 30° from the first of the causes above named. This would amount to from 12 to 25 per cent. upon the total theoretic consumption. In working with $l = m$, it would be 20 per cent.; and this alone would make a considerable reduction upon the favorable ratios in the table.

It will be seen, by inspection, that if power is sought without regard to economy, large supply cylinders are to be preferred. The construction proposed by the present writer in the last number of this Journal, renders the use of such cylinders practicable, though it would not be so on Ericsson's plan.

If economy is sought at the expense of power, cylinders from ·60 to $\frac{2}{3}$ in cross section, furnish the best results. In this point of view, Capt. Ericsson's proportions are well chosen. It is also apparent that the cut-off which produces the most economical results is at ·75 or thereabouts, whatever be the ratio between the cylinders.

But, on all accounts, it is evident at a glance, that the supply cylinders between ·70 and ·80 in section (probably about ·75 in preference to any other) are most eligible. Very little is lost by their use, in point of economy, while there is secured a very great gain of power.

But here probably we encounter the great and it is to be apprehended the inseparable obstacle, in the way of the success of the hot-air engine viz., the practical difficulty of heating the great mass of air with sufficient rapidity. Suppose we make all reasonable allowances for the effect of friction, leakage, and clearance of cylinder, it is not possible after all to bring down the power so low as the experiment with the "Ericsson" ship showed it in fact to be.

Capt. Ericsson, for instance, says that a pressure of half a pound to the inch, is sufficient to overcome resistances. At 12 pounds above the atmosphere in the reservoir (what he aimed at), this would constitute a charge of about 15 per cent. upon the power; for with 12 pounds actual pressure, but about 3·4 lbs. pressure over resistances is obtained. For leakage we have no reliable data upon which to form an estimate; but we know that in the

ship, the leakage was actually large, and it will probably be not far from the truth to put this at 15 per cent. more. We thus obtain a reduction of thirty per cent. in all, while the engines fell as far as fifty per cent. below their aggregate theoretic power.

The remaining deficiency can hardly be accounted for, but by supposing that the air *could not be heated* by furnaces and regenerators both together, to a sufficient degree and with sufficient rapidity to maintain the theoretic pressure, and the assumed velocity of piston. This explains the statements made by reporters present on the trial trip, that the temperature of the working cylinder did not rise above 440°, and also Capt. Ericsson's repeated assertions that he could not carry more than 8 pounds of pressure. And this may satisfactorily explain the published statement which has remained for two months uncontradicted, that the original plan of heating the air has been entirely abandoned.

The results to which this investigation has brought us, are less encouraging than were anticipated, or than, in the beginning we even desired to find them. We are compelled to look, at present, much less hopefully upon this invention than we have been disposed to do heretofore; so much less so, as to apprehend that it can never come into successful use as a motive power.

Univ. of Alabama, Aug. 30, 1853.

ART. XXXVII.—*On the Consolidation of Coral Formations;* by JAMES D. DANA.

At the Cleveland meeting of the American Association for the Advancement of Science, in August last, Prof. Horsford read a labored reply to the writer's criticisms* of his views, on the consolidation of coral limestone. This paper by Prof. Horsford, has appeared in several daily newspapers, and has thus had a wide circulation. Deeming the public papers no proper place for scientific controversy, either for attacks or replies, and knowing that an author may not hold himself responsible for the accuracy of articles thus published, the writer had intended to take no notice of the arguments of Prof. Horsford, until they had appeared elsewhere. But on finding evidence in the date of the reading of the paper at Cleveland, (Friday, July 29,) and its appearance in the Boston Traveller, (Monday, Aug. 1,) that the article was in type before it was read, and that therefore, it may have been set up long enough before the meeting of the Association to allow of Prof. Horsford's correcting the proofs,† there appeared to be suffi-

* This Journal, [2] xiv, 410.

† We might give further evidence on the above point, but deem it unnecessary. The paper occupies 5½ columns in the Traveller, each about two feet long.

We understand that Prof. Horsford considers us as having refused the publication of any reply from him. We wrote him that we should submit his reply, when sent

cient reason for considering Prof. Horsford responsible for what he had thus published, and for subjecting his statements to a brief notice in this place. I was not present at the meeting of the Association where the paper was read.

I propose therefore, to touch briefly upon some of the more obvious errors, misapprehensions, or inadvertencies, into which Prof. Horsford appears to have fallen.

I. Prof. Horsford's investigations were made principally upon what he calls the "crust-rock," one of the two kinds of coral formations which he mentions; and this might be supposed from his descriptions to be a common form of the coral reef rock. In fact, it is an uncommon variety. I found nothing like it in my explorations among the Pacific reefs and islands. The ordinary submerged reef rock, the prevailing rock wherever there are reef-regions, has no resemblance whatever to the "crust-rock," and is distinct also from his "oolitic" rock. It is sufficient to account for the misapprehension which Prof. Horsford appears to be under respecting reef rocks, to state that he has never seen a coral reef.

II. Some examples of a thin calcareous crust, are described by the writer as occurring in the Sandwich Islands. Prof. Horsford errs in regarding these as examples of his "crust-rock". The author has described them as a surface formation upon elevated hills of coral sand rock; that is, as mere surface crusts *upon the "oolitic" variety* of Prof. Horsford; it is of much more modern origin than the rock itself, and not one of the two prevailing varieties of reef rocks, any more than a stalagmite is an example of the rock which it encrusts.

III. Prof. Horsford supposes erroneously that the explanation of the origin of this crust which I give, has been applied by me to the West India "crust-rock." Not having seen that rock in place, I have offered nothing upon its origin or consolidation.

IV. Prof. Horsford proceeds in his theory upon the supposition that the coral sands of seashores and submerged coral deposits of reef grounds, contain a large per-centage of animal matter. This conclusion, he has not sustained by an examination of these materials from various regions: he is satisfied after an examination simply of some specimens of his "crust-rock;" and proceeds at once to theorise for all coral formations: for if he does not say in so many words that his theory is intended for coral rocks generally, the manner in which he has replied to my objections, and the use

us, to Dr. Gibbs, our collaborator in charge of chemistry and physics, and that if in his opinion its chemistry and statements of facts were correct, it would be published; also, that if his former paper had been thus submitted, it would not have appeared in this Journal, on the ground of its incorrect chemical principles. Had his recent reply been sent us, we should probably have saved Prof. Horsford the publication of some of its errors.

he makes of the facts I have published, show that he intends for it this universal application.

V. In the analyses of the "crust-rock" in Prof. Horsford's former paper, he claimed to have found in one case, 20 per cent. of animal matter. In the recent paper, another analysis is given in which there are but 1·47 per cent., (no more than was found by Prof. Silliman, Jr. in some of the bleached corals,) and the difference is accounted for, on the ground that the "stage of decomposition of the organic matter was further advanced" than in the specimens before examined. This recent analysis is therefore no confirmation of the former results: moreover, the discrepancies in the earlier analyses, pointed out in my former paper, have shown them to be not altogether trustworthy, and Prof. Horsford now admits that the object of the first analysis was qualitative rather than quantitative. The foundation for the theory proposed, is therefore based on a scanty supply of imperfectly ascertained data.

VI. In discussing the chemical objections to his theory, he comes at last to the point on which the whole turns,—the formation of hydrate of lime,—and thus observes:

"Prof. Dana proceeds: But suppose the uncombined ammonia to be formed during the decomposition, will this ammonia precipitate the hydrate of lime from the solution of the sulphate? It is perhaps sufficient to say in regard to this statement of Prof. Dana's, that it was written in entire misapprehension. It is due to myself however to add, that I have never uttered anything of the kind, and that in an earlier allusion to the view, the learned author correctly states what I did say. He says in repeating my views. 'The ammonia resulting from the nitrogen, carries off the sulphuric acid [of the sulphate of lime]* as sulphate of ammonia, and leaves the lime as a SOLUBLE *hydrate*.'"

The small capitals and italics are Prof. Horsford's.

The assertion, "I have never uttered anything of the kind," is forcible and apparently decisive.

The verbal difference between the statement alleged to be erroneous, and that acknowledged to be correct, consists in substituting *precipitate* for separate, and in omitting the word *soluble*.

Judging from the peculiar stress laid upon the word soluble by the use of capitals, it might be supposed that Prof. Horsford believed in a SOLUBLE hydrate, as distinct from another which was *insoluble*.

But chemists know of but one, and that takes 778 parts of water, at 60° F. (according to Dalton) to dissolve *one* part, or it is about *one half* as soluble as sulphate of lime or gypsum:† in pro-

* Prof. Horsford in his former paper, uses the words, "The nitrogen going over into the form of ammonia at a later period, decomposed the sulphate of lime, forming sulphate of ammonia and soluble hydrate of lime." We therefore add, "of the sulphate of lime," to make the statement complete.

† According to Bucholz, gypsum dissolves in 460 parts of water, either hot or cold, according to Giese, in 380 parts of cold and 388 parts of boiling water.—*Gmel. Chem.* iii, 202.

portions larger than this, it would be *precipitated* whenever *separated*. The two statements would therefore seem to a chemist to be very much of a kind, if not identical,—unless it is meant that more than *one* part of the hydrate to 778 of water would never be separated.

We pass to the chemistry of the passage which is the important point in the whole discussion,—for if false, the theory proves to be no better in its principles, than in its alleged facts.

Now it is well known that *ammonia will neither separate, nor precipitate, soluble or insoluble hydrate of lime, by action on the dissolved sulphate;* on the contrary, sulphate of ammonia is readily decomposed by a salt of lime, or by its hydrate. What avails then the long array of facts and arguments with which Prof. Horsford fills up his paper? Much might be brought forward on other parts of his chemical discussions, but as the keystone of his theory is gone, it would be a waste of words.*

VII. Prof. Horsford repeats from his former paper another mode of consolidation, viz., "The deposition of finely powdered carbonate of lime, with mucilaginous matter filling up the interstices between the grains of rock, and serving to increase the cohesion." This idea of making rocks by sticking the particles together with mucilage, (or with glue, as is implied in another place,) is quite original with Prof. Horsford. It needs no discussion.

VIII. After going over, with many details, the prominent points in the theory of consolidation proposed by him, Prof. Horsford presents another view which was barely intimated in his former paper, and which falls in with the theory that I had been led to adopt.

Before remarking upon it, I would state here that I have myself brought forward no *original* theory on this subject, but instead, have only appealed to a principle more or less fully received; —that of consolidation through the carbonate of lime dissolved in the carbonated waters which penetrate the deposited coral mud or sand. The beach sands and the drift heaps formed by the winds,—or in a word, all accumulations above low tide level,—are alternately wet and dry, under the action of the sea, and also the occasional rains. The rains are known to contain some carbonic acid, and the sea water also has been found to include a small proportion, so as to be able to dissolve some carbonate of lime, and, in time, produce the consolidation. With reference to submerged deposits, the same principle was supposed to operate; but as there

* Prof. Horsford insists in his article upon a certain order of decomposition, which is necessary to his theory; sulphuretted hydrogen being an early step, and ammonia or an ammoniacal salt the later. Facts in nature are the reverse of this. Moreover, he supposes the gas from animal decomposition, often called sulphuretted hydrogen from its smell, to be actually this gas or mainly so and uncombined, which again in many cases at least is not true.

is no *drying* in the process, it was suggested with hesitation that there might be a chemical action under the influence of the magnesian salts of the ocean, leading to a double carbonate of lime and magnesia, that is, a *dolomisation*,—Haidinger and von Morlot having shown that such a chemical change may be produced, at least under the influence of heat, and B. Silliman, Jr., having found a specimen of reef-rock to be a magnesian carbonate of lime.* The author has acknowledged that the subject required further investigation. It is possible that the superincumbent pressure may be one agency in promoting consolidation in the submerged deposits. It is altogether probable, that the process of animal decomposition going on within these beds, tends to promote, through the play of molecular forces thus set in action, (a kind of catalytic or "presence" influence,) the solidification of the mass by the crystallization of the carbonate of lime in solution,—a play of forces of one kind inducing the molecular action resulting in cohesion; and the same circumstances or condition would be particularly favorable for chemical changes, like that of dolomisation.

Prof. Horsford deserting in part, his favorite theory, here takes the old ground that consolidation may result through the carbonic acid in the permeating waters, and adds the suggestion that the *carbonic* acid might be derived from the decomposition of the animal matter within the coral mud,—which appears to be a sensible idea.

IX. In discussing this last-mentioned point, he says: "The slowly evolved carbonic acid would give to the surrounding pulverulent carbonate of lime, if not perfect solution, a degree of mobility which would permit the larger grains of carbonate of lime to attract it to themselves, and thus yield a kind of cement;" and again, "The carbonic acid would give mobility to the carbonate of lime, which would take on, according to the perfection of the solution, more or less of crystalline form." We confess we do not understand this "mobility." Infusorial mobility is out of the question; and what else shall we suppose was intended? The developed carbonic acid might shove the particles first one way, and then another, but this is not a consolidating process. If by *mobility* it is meant, that the waters thus carbonated would dissolve some of the carbonate of lime, and so give the particles mobility, and thereby promote consolidation, it is the old idea, though strangely expressed. If he means (seemingly he does not) that the process of decomposition is one promotive of consolidation, by a sort of catalytic influence, as above remarked, it might have been more distinctly explained.

* Dana's Exp. Exp. Geol. Report, p. 153

X. The absence of carbonic acid from sea-waters is a point which Prof. Horsford endeavors to prove by reference to several analyses. But is it true that carbonic acid is evolved as he allows, from the decomposition of animal matter in the mud or sands, forming the bottom of coral-reef regions, and yet that none escapes, so as to be found in the waters of these regions? It is obviously impossible.

Many analyses have actually found the carbonic acid or carbonate of lime. To meet the case fully, the analyses should be made of waters from reef regions, and not of those from the open sea. But Darondeau and Henry have found carbonic acid even in the air dissolved in various sea waters at different depths; and as their results are of some general interest, we cite the following table from the *Annales de Chemie et de Physique*, lxix, 103. The latitudes and longitudes are reckoned from Paris.

Time collected.	Lat.	Long.	Depth.	Density at 8° & 10° C.	Gas in 100 parts of water, at 0° C., & 760 mm. pressure.	100 parts of gas.		
						Oxyg.	Nitrogen	Carb. acid
Aug. 30, '36	11° 8′ N.	108° 50′ W	*Surface*	1·02594	2·09	6·16	83·33	10·51 (?)
"	"	"	70 *fath.*	1·02702	2·23	10·09	71·05	18·06
Mar. 19, '37	11° 43′ N	87° 18′ E.	*Surface*	1·02545	1·98	5·53	80·50	13·97
"	"	"	200 *fath.*	1·02663	3·04	3·29	38·56	58·15
May 10, '37	18° 0′ N.	85° 32′ E.	*Surface*	1·02611	1·91	6·34	80·34	13·32
"	"	"	300 *fath.*	1·02586	2·43	5·72	64·15	30·13
July 31, '37	24° 5′ S.	52° 0′ E.	*Surface*	1·02577	1·85	9·84	77·70	12·46
"	"	"	450 *fath.*	1·02739	2·75	9·85	53·23	31·92
Aug. 24, '37	30° 40′ S.	11° 47′ E.	400 *fath.*	1·02708	2·04	4·17	67·01	28·82

XI. Prof. Horsford in his former paper, attributes the coral mud of coral islands "in part," (I did not intentionally suppress this qualifying expression in my citation of his views,) to "the stone plants which flourish in the waters within the reef, and which admit of ready reduction to extreme fineness." In mentioning my objection to this view, he says:—"It is unfortunate that we find the author (Prof. Dana) elsewhere remarking: 'The Nullipores, properly calcareous vegetation, (stone plants,) flourish best along the line of breakers and form *thick accumulations upon the reefs.*'"

There are two misapprehensions here. First, Nullipores, and especially those that flourish in the breakers, are generally *less fragile* than corals themselves, being in solid calcareous branching forms, or incrustations, and always without cells. Secondly, the accumulations alluded to in the quotation from my Report, are absolutely solid massive limestone, and *not deposited grains or mud*, and they arise from successive growths over one another of these incrusting stone-plants.

Prof. Agassiz found in the West Indies, large accumulations of fragments of tender calcareous Algæ; and these Prof. Horsford evidently had in mind: but a calcareous mud is almost as readily made by the moving waters out of the fragments of coral as of these Algæ.

XII. To the principle, abundantly exemplified, that on shores where the waves break heavily, beds of pebbles or sand are produced, since the waters convey off the finer particles, and leave only the coarser; and on those where the motion of the waters is gentle, trituration produces a mud more or less fine, because the triturated material is not only ground up fine, but is deposited together where it is ground up; Prof. Horsford observes, giving to the theory a rule-of-three form, which is entirely his own: "As an application of the theory, that the less the agitation the finer will be the powder, the writer (Prof. H.) asks, *what* degree of fineness would be obtained where water is entirely at rest?"—a profound problem, which may be paralleled, and perhaps elucidated in some slight degree, by the enunciation of another of equal significance, bearing upon the laws of motion in general:—If the slower a coach moves the longer it will take to go a mile, how long will it take if it does not go at all?

XIII. Prof. Horsford passing from reef-rocks to coral polyps, suggested with reference to the secretion of carbonate of lime that "the carbonate of lime of the coral, was the fruit of simple double decomposition of the sulphate of lime in sea water, by the carbonate of ammonia exhaled from the living coral." To this suggestion I objected in my former paper, on the ground that the excretions could not turn about and aid in the secretions.*. He now says, "it appears to me that the precipitate of carbonate of lime under the circumstances supposed, would take place in the walls of the cellules, where the carbonate of ammonia is set free, and would of course take the form of the cellules:"—thus making the secreting cellules produce, contrary to all that is known, carbonate of ammonia, one of the results of the ultimate decomposition of animal matter.

XIV. The deposition of carbonate of lime from calcareous waters is the last point which I will now mention. Prof. Horsford commences his observations on this subject as follows:

"He (Prof. Dana) remarks: 'Among the modes of consolidation of limestone or carbonate of lime, the following are enumerated by Prof. Horsford; 1, the common mortar process; 2, that of hydraulic cements; 3, that of deposition from calcareous springs.' Of the last he says: 'The waters containing carbonate of lime held in solution by an excess of carbonate of lime, upon reaching the surface under less pressure, and the influence of a high temperature (*higher* temperature, in the manuscript, E. N. H.) give up the carbonic acid so that a precipitation of the carbonate of lime takes place. But is this pressure or a

* I have stated in my report, that the coral polyps have probably the power of deriving the lime by secretion from the sulphate of lime present in the waters; but I have not undertaken to point out the exact character of the change, as the question has not been investigated.

higher temperature needed? The common deposition of carbonate of lime from the waters dripping through the roofs of caverns is evidence to the contrary.'

"I quote the whole literally, with its various inaccuracies to illustrate the haste with which the article was penned.

"The pith of the criticism is the gratuitous intimation that diminished pressure and elevated temperature are not required for the escape of carbonic acid, in order to the precipitation of carbonate of lime held in solution by carbonic acid."

He goes on to illustrate by reference to *soda water*, etc. In another place, after mentioning the remark of the writer, he says, with point, "The reply to this may be found in elementary works on chemistry."

I do not care to damage by my remarks any elementary work on chemistry. But how does the case stand? Prof. Horsford is right as to the pith of my criticism, and also as to my opinions being given gratuitously. Now suppose we collect some of the water dripping from the roof of a cavern; and suppose we cool it down at once from 60° F. to 40° F., and allow such a wind as occurs often at the Mammoth Cave to blow over it, would it not evaporate, and the carbonate of lime be thereby deposited? Suppose again the barometric pressure upon that water could be increased from *one* atmosphere to *two*, would not the winds still produce evaporation and a deposition of the lime? There can be only an affirmative reply to these questions. All that is needed is *evaporation*, and this may take place with a *diminished* temperature and *increased* pressure. If we should now take that water and attempt to deposit the carbonate of lime by means of heat, it would be necessary according to such elementary treatises of chemistry as we have seen, to raise the heat to the boiling point of water, before the carbonic acid would be entirely driven off, and the carbonate of lime which it contains, would be deposited.*

In conclusion, I would wish Prof. Horsford better success in his further researches upon coral formations; I doubt not that whatever he may offer which shall throw actual light on the subject, will be gladly welcomed by all. I have entered into this discussion only because erroneous statements unanswered are received by many as truths; and once adopted, they are apt to cling most pertinaciously to the science. The chemistry of coral or calcareous formations is a fair field for research, and important developments will no doubt reward careful study.

* As to the "various inaccuracies" above, on which a grave charge is rested, there is but one which the author has discovered, that of writing carbonate of lime for *carbonic acid:* we could point out almost the same error in the paper we are reviewing; but we think it degrading to descend to such trifles in a scientific discussion. The error of high for *higher*, moreover, is Prof. Horsford's, as he acknowledges; it was printed *high* in the Proceedings of the Albany meeting of the Association, where his article first appeared; and in the *copy* of the paper, *revised by him* for this Journal, where it was published at his request, the error remains uncorrected.

ART. XXXVIII.—*Reëxamination of American Minerals: PART III—Danburite; Carrollite; Thalite; Hudsonite; Jenkinsite; Lazulite; Kyanite; Elæolite; Spodumene; Petalite:* by J. LAWRENCE SMITH, M.D., and GEORGE J. BRUSH, Ph. B.

27. *Danburite.*

THIS mineral has as yet been found in this country only at one locality, Danbury, Conn. It was first described by Prof. C. U. Shepard,* and considered by him a hydrated silicate of lime and potash. Still later it was examined by Dr. H. Erni,† and boracic acid was found to be an important ingredient in its constitution; in his analysis, however, a large amount of alkalies is indicated. As the results of our analyses show but a trace of these substances and no other base but lime, it is possible that Dr. Erni's alkali determination was made, through mistake, on some of the feldspar accompanying Danburite, as it not unfrequently happens that the granular portions of the feldspar resemble the lighter varieties of Danburite. This supposition appears reasonable, as the silica and lime of his determinations agree with those about to be stated.

The composition as given by the authorities mentioned are,

$\ddot{S}i$ $\dot{C}a$ $\ddot{A}l$
56·00 28·33 1.70 $\dot{Y}$? 0·85 $\dot{K}$ (with $\dot{N}a$?) and loss 5·12 $\dot{H}$ 8·0 =100. *Shepard.*
49·74 22·80 $\dot{M}g$ 1·98 $\dot{F}e$ and $\ddot{A}l$ 2·11 $\dot{K}$ 4·31 $\dot{N}a$ 9·82 $\ddot{B}$ 9·24 =100. *Erni.*

The results of the analyses just made, are as follows:

	1.	2.	3.	4.
Silica, . .	48·10	48·20	——	——
Alumina, }	·30 }	1·02	——	——
Peroxyd of iron, }				
Manganese, .	·56 }			
Lime, . .	22·41	22·33	22·22	22·11
Magnesia, .	·40	*undeterm.*	——	——
Boracic acid, .	27·73	27·15	——	——
Ignition, . .	·50	·50		
	100·00	99·20		

This corresponds to the following constitution,

	Pr. ct.	Ox. ratio.
4 ats. Silica, ...	49·42 .	4
3 " Boracic acid, . . .	28·02 .	3
3 " Lime, ...	22·56 .	1

It is not easy to decide upon the formula by which Danburite is to be expressed. The same difficulty occurs here as in the case of datholite; Berzelius considering the lime only as acting the part of base, while Rammelsberg regards the boracic acid also as performing the part of a base. The formula for Danburite under these views would be expressed by

$$2\dot{C}a\,\ddot{S}i^2 + \dot{C}a\,\ddot{B}^2 \quad \text{or} \quad \dot{C}a^3\,\ddot{S}i^4 + \ddot{B}^2\,\ddot{S}i^3$$

* See Am. Jour. Sci, [1] xxxv, 129. † Am. Jour. Sci., [2] ix, 286.

We are in favor of regarding the latter as representing the true formula.

If Danburite be examined in its relation to datholite, it will be found to differ from the latter in having just one half the number of atoms of lime minus three atoms of water.

$$\dot{C}a^6\,\ddot{S}i^3 + \dot{B}^2\,\ddot{S}i^3 + 3\dot{H} = \text{Datholite.}$$
$$\dot{C}a^3\,\ddot{S}i^2 + \dot{B}^2\,\ddot{S}i^3 \quad = \text{Danburite.}$$

This mineral forms then the second natural borosilicate of lime. Its physical characters have been fully described elsewhere.

The results of the analysis somewhat surprised us, as we confidently expected to find alkalies; of course much care was directed to the settling of this question, which from the simplicity of the composition of Danburite was easily done in the following manner. To one gramme of the mineral, strong hydrofluoric acid was twice added, the mass carefully heated and evaporated nearly to dryness on each addition; finally, sulphuric acid was added in excess, the whole evaporated to dryness and ignited; the residue weighed 563 milligrammes, of which 544 were sulphate of lime, the remaining 19 being composed of alumina, iron, manganese, and magnesia. The 544 milligrammes of sulphate of lime, correspond to 22·44 pr. ct. of lime in the mineral used, proving, clearly that the remainder was all volatilized under the action of hydrofluoric acid; which according to previous experiments was shown to consist of silicic and boracic acids. Three repetitions of the above experiment gave perfectly concordant results: in fact no more beautiful proof could have been had of the absence of all other bases but lime, in any very sensible quantity. Moreover the amount of lime thus indicated, agrees perfectly with that obtained by the direct method of analysis.

The boracic acid was estimated by first ascertaining the exact amount of silica by a soda fusion, and then deducting this quantity from the entire loss of a given quantity of the mineral under the action of hydrofluoric acid.

The specimens examined were among the finest that have ever been found, and were procured by Mr. Brush at the locality.

28. *Carrollite, a Copper-Linnæite.*

This mineral occurs at Finksburg, Carroll Co., Maryland, and was described as a new species by Mr. W. L. Faber.* He gave as its chemical composition,

S	Co	Ni	Cu	Fe	As	$\ddot{S}i$
27·04	28·50	1·50	32·99	5·31	1·82	2·14

Formula $2Co\,S + Cu^2S$.

Our attention was first called to this mineral from the unusual relation of the sulphur to the metals in its composition, it being

* This Journal, 2d Series, vol. xiii, 418.

we believe the first example of a natural subsulphuret that had been observed, the relation being R^4S^3.

Having been furnished through the kindness of Prof. Dana, with an abundance of the mineral to select from, which he had procured from original specimens in the hands of Prof. Booth, it was carefully separated from the copper pyrites with which it is associated. A reëxamination of it gives the following for its physical and chemical characters. Hardness 5·5; sp. grav. 4·85.* Lustre metallic; color steel gray; fracture uneven, without sufficient indication to make out clearly the nature of its cleavage. The results of three analyses are

	1.	2.	3.
Sulphur,	41·93	40·94	40·99
Cobalt,	37·25	38·21	37·65
Copper,	17·48	17·79	19·18
Nickel,†	1·54	1·54	1·54
Iron,	1·26	1·55	1·40
Arsenic,	trace	trace	trace
	99·46	100·03	100·76

These correspond to the general formula

$$RS + R^2S^3$$

as will be seen by comparing the amount of sulphur required for the metals indicated in the three analyses with the quantities found.

	1.	Sulphur.	2.	Sulphur.	3.	Sulphur.
Cobalt,	37·25	27·00	38·21	27·68	37·65	27·21
Copper,	17·48	11·65	17·79	11·88	19·18	12·78
Nickel,	1·54	1·12	1·54	1·12	1·54	1·12
Iron,	1·26	·93	1·55	1·15	1·40	1·03
		40·70		41·83		42·14

Substituting cobalt for the copper, iron and nickel, the entire amount of the cobalt would be represented in the three analyses by 56·37, 57·92, and 58·50 pr. ct. The formula requires

	Atoms.	Pr. ct.
Sulphur,	4	42·06
Cobalt,	3	57·94

This is the formula and constitution of Linnæite, and the mineral in question is a copper Linnæite similar in composition to the one from Riddarhythan, Sweden.

The composition of this mineral is interesting, as it furnishes the only example in the mineral kingdom of the isomorphism of copper and cobalt, where the latter may be replaced to a greater or less extent by the former. Among artificial products exhibiting this replacement, we have the cupro-sulphate of cobalt with 10 and with 7 atoms of water, the latter crystallizing in oblique prisms, like the sulphate of cobalt with the same number of atoms of water, which is also the form of green vitriol.

* It may be well to remark that there is a typographical error in the statement of the sp. grav. of Linnæite in Dana's Mineralogy; it is never above 5·00.

† Only one estimate of nickel made.

29. *Thalite, identical with Saponite.*

This mineral was originally described by Dr. D. D. Owen,* by whom it was found on the north shore of Lake Superior, diffused in the amygdaloidal traps of that locality. At the time it was first noticed, it was supposed to contain a new element, which was called thalium; the mineral itself was named thalite. Through the kindness of Dr. Owen we were furnished with some of the mineral, which was subjected to most careful analysis, the result showing nothing in its composition by which it differs from saponite; and all attempts to isolate a new earth from it were vain. A second portion of the thalite, with some of the supposed thalia was sent us by Dr. Genth of Philadelphia, which was labelled, "not quite pure;" its composition, however, differed from the first principally in containing less water, as it was allowed to dry for a greater length of time—it being a common thing for saponites to lose more or less of their water by desiccation in the air.

The result of the examination of the thalite was given in a note in the last number of this Journal. Many of the reactions contained in the original description of thalite and thalia we have been unable to recognize, among them the evolution of chlorine by the action of hydrochloric acid, and the precipitation by a neutral solution of succinate of ammonia. The pea-green color of the concentrated hydrochloric acid solution of the thalia, prepared in the way mentioned by Dr. Owen, is easily explained by the presence of an exceedingly minute quantity of the chlorid of chromium, as the smallest trace of this last metal will, under the circumstances, produce that color.

The results of our analyses are as follows:

	1.	2.
Silica,	45·60	48·89
Alumina,	4·87	7·23
Oxyd of iron,	2·09	2·46
Manganese,	*trace*	*trace*
Lime,	1·07	—
Magnesia,	24·10	24·17
Soda, Potash,	·45	·81
Water,	20·66	15·66
	98·84	99·22

The specimen No. 1, as already stated, came directly from Dr. Owen, No. 2 from Dr. Genth. The original analysis of Dr. Owen, with the exception of the new earth, does not differ very materially from the above, when we consider that the saponites vary more or less in their composition. It is as follows:

$\dot{S}i$	$\dot{A}l$	$\dot{F}e$	new earth	$\dot{M}g$	$\dot{K}$	Mn	$\dot{H}$
42	4·6	1·5	10–12	20·5	0·8	trace	18

* Jour. Acad. Nat. Sci. Philad., ii, part 2d, 1852.

If then the existence of a new earth in this mineral cannot be established, it is clear that it must be a saponite, with which mineral it is identical in physical properties.

30. *Hudsonite, a Pyroxene.*

This species was described by Prof. L. C. Beck.* It has since been shown to be a pyroxene in which a portion of the silica is replaced by alumina. Beck and Brewer obtained for its composition,

Si	Al	Fe	Mn	Mg	Ca	
36·94	11·22	36·03	2·24	——	12·71 = 99·14.	*Brewer.*†
37·90	12·70	36·80	——	1·92	11·40 = 100·72.	*Beck.*

A recent examination of some specimens has shown the presence of a considerable amount of alkalies. The mineral was received from Mr. Silas R. Horton, and is the same as was sent by him to Prof. Beck. The results of two analyses are,

	1.	2.
Silica,	39·30	38·58
Alumina,	9·78	11·05
Protoxyd of iron,	30·40	30·57
" Manganese,	0·67	0·52
Lime,	10·39	10·32
Magnesia,	2·98	3·02
Potash,	2·48 }	4·16
Soda,	1·66 }	
Ignition,	1·95	1·95
	99·61	100·17

Considering the alumina as replacing silica, these give the oxygen ratio of pyroxene and the formula $\dot{R}^3(\ddot{S}i, \ddot{A}l)^2$.

31. *Jenkinsite.*

The green mineral that occurs in velvety coatings on the magnetite of O'Neil's mine, in Orange County, N. Y., has been described by Prof. Shepard,‡ as a new species. Its intimate association with magnetite, renders it somewhat difficult to obtain it perfectly pure; but by placing the fine particles of Jenkinsite in a vessel of water, and stirring the mass with a clean rod of soft iron, that passes through an electro-magnetic coil in connection with a battery, every particle of magnetic iron is removed. Two different portions thus purified, procured at different times from Mr. Silas R. Horton and Mr. John Jenkins, gave

	1.	2.
Silica,	38·97	37·42
Protoxyd of iron,	19·30	20·60
" manganese,	4·36	4·05
Magnesia,	22·87	22·75
Alumina,	0·53	0·98
Water,	13·36	13·48
	99·39	99·28

* Mineralogy of N. York, p. 310. † From Dana's Mineralogy, 3d ed., p. 267.
‡ This Journal, [2] xiii, 392.

From these we obtain the mean oxygen ratio for silica and protoxyds, and water, 19·84 : 14·50 : 11·92 = 4 : 3 : 2½, and the formula $\dot{R}^3\ddot{S}i^4 + 7H$.

	Atoms.	At. wt.		Pr. ct.
Silica,	4	2309	=	38·83
Magnesia,	6	1500		25·23
Protoxyd of iron,	3	1350		22·70
Water,	7	788		13·24
		5947		

The mineral is similar in composition to serpentine with one atom more of water, and the magnesia replaced in part by protoxyd of iron and manganese. It also has a strong resemblance to *hydrophite*, both in chemical and physical properties.

32. *Lazulite.*

This species occurs in considerable abundance in Sinclair Co., N. C., which is its only American locality. It is of interest to compare its composition with the European varieties, and for that reason the examination was made.

The sp. gravity was 3·122. Two analyses gave

		Oxygen.				Ox.		
Phosphoric acid,	43·38	= 24·31			44·15	= 24·74		
Alumina,	31·22	14·59			32·17	15·03		
Protoxyd of iron,	8·29	1·84	}	5·86	8·05	1·79	}	5·80
Magnesia,	10·06	4·02	}		10·02	4·01	}	
Water,	5·68	5·05			5·50	4·89		
Si,	1·07				1·07			
	99·70				100·96			

No. 1, has the oxygen ratio = 24·90 : 14·94 : 6 : 5·16, or very nearly 25 : 15 : 6 : 5.

No. 2, has = 25·56 : 15·54 : 6 : 5·04.

From these we deduce the formula

$$2(\dot{M}g, \dot{F}e)^3\ddot{P} + \ddot{A}l^5\ddot{P}^3 + 5\dot{H}$$

	Atoms.	At. wt.	Pr. ct.
Phosphoric acid,	5	4460	44·02
Alumina,	5	3209	31·67
Protoxyd of iron,	2	900	8·88
Magnesia,	4	1000	9·87
Water,	5	563	5·56

The formula differs from that of Rammelsberg, by one atom less of alumina and of water; calculated by his formula it would give the alumina much too high for our analyses. The phosphoric acid was separated from the alumina, by fusing the mineral with carbonate of soda and silica, this being the most perfect method, in fact the only one to be safely relied on. It appears to be identical in specific gravity and composition with the variety from Gratz examined by Rammelsberg.

33. *Kyanite.*

Associated with the lazulite just described, is a very beautiful white kyanite. Its composition is

Silica,	37·60
Alumina,	60·40
Peroxyd iron,	1·60
	99·60

This corresponds to the formula $\dot{A}l^2\ddot{S}i^2$ = silica 37·47, alumina 62·53.

34. *Elæolite.*

The Elæolite of Magnet-Cove in Arkansas, passed under the name of "flesh red feldspar" until recognized by Prof. Shepard.* It has the following physical and chemical properties. Hardness 6. Sp. gr. 2·65. Color flesh-red. Lustre greasy. Structure massive. Chemical composition,

Silica,	44·46
Alumina,	30·97
Peroxyd of iron,	2·09
Lime,	0·66
Soda,	15·61
Potash,	5·91
Ignition,	0·95
	100·65

From this we have the oxygen ratio for the silica, peroxyds, and protoxyds, 9 : 6 : 2, and the formula $\dot{R}^2\ddot{S}i + 2\dot{A}l\ddot{S}i$. The mineral examined, was furnished by Mr. Markoe of Washington; it is the one alluded to in our last paper, as containing the compact Thomsonite, under the name of ozarkite. Since publishing the analysis of the latter, we have procured a specimen of the elæolite containing the Thomsonite in handsome radiated crystallisations.

35. *Spodumene.*

Several analyses of this species from Norwich and Sterling, Mass., were given by Mr. Brush in vol. x, of this Journal.

In these analyses, the alkalies were determined as sulphates, and from the amount of sulphuric acid, the relative amount of alkalies was calculated. Since these examinations, it has been found that this process is liable to great inaccuracy; and we have consequently made a reëxamination with direct reference to this point; it has been made altogether on the variety from Norwich, which occurs in beautiful crystals. The results of the reëxamination show that the lithia and soda determinations in the analyses referred to, are erroneous. The method used in this in-

* This Journal, [2] ii, 252.

stance is that recommended by Rammelsberg, the separation of the chlorid of lithia from the chlorids of potash and soda, by a mixture of alcohol and ether.

These analyses gave

	1.	2.	3.
Silica,	64·04	63·65	63·90
Alumina,	27·84	28·97	28·70
Peroxyd of iron,	0·64		
Lime,	0·34	0·31	0·26
Magnesia,	*tr.*	*tr.*	*tr.*
Lithia,	5·20	5·05	4·99
Soda,	0·66	0·82	0·80
Potassa,	0·16		
Ignition,	0·50	0·50	0·60
	99·38	99·30	99·25

From these is deduced the oxygen ratio for the protoxyds, peroxyds, and silica, 1 : 4 : 10 and the formula $\dot{R}^3\ddot{S}i^2+4\dot{A}l\ddot{S}i^2$, corresponding with the results recently obained by Rammelsberg,* for the composition of the spodumene from Uton and the Tyrol. The silica, however, is somewhat lower than that of the European specimens, which is probably owing to the greater purity of the American mineral, it being found in crystals.

Since completing the above examination of the spodumene from Norwich, we have noticed a recent examination of the variety from Sterling, Mass., by Rammelsberg in which he states his having found 4·5 pr. ct. of potash, and a ratio nearer 1 : 5 : 12, than 1 : 4 : 10. This difference is accounted for by that author on the ground that the mineral is somewhat decomposed, and that the original ratio was 1 : 4 : 10. The fact is, Rammelsberg did not examine the pure spodumene from Sterling, as will be seen by his own account of the specimens analyzed. They were white, yellowish or blueish grey in color, possessing little lustre, penetrated with small fissures, and in these fissures as well as in many places on the surface, were scales of mica with yellow flakes of oxyd of iron ; the specific gravity was 3·073. Now the spodumene, as we have procured it perfectly pure from Sterling, Mass., is of a greenish grey color, fine lustre, and very compact, cleaving with perfection, a specific gravity of 3·182, and with a composition according perfectly with the requirements of the ratio 1 : 4 : 10, containing even a little more lithia than the variety from Norwich. In an early examination by Brush, published several years ago, the silica was given too low, he not having separated the small amount of silica always associated with the alumina. A recent analysis gives as the composition of this spodumene :—

* Pogg. Ann., lxxxv, 544.

			Ox. ratio.
Silica,	64·50		33·41 or 10·66
Alumina,	25·30		
Peroxyd of iron,	2·55		12·54 or 4
Lime,	0·43		
Magnesia,	0·06		
Lithia,	5·65		3·43 or 1·09
Potash and soda,	1·10		
Loss by ignition,	0·30		
	99·89		

It will be seen that this forms no exception, and when examined pure, has the same composition as spodumene from other localities. In fact, so far as the American spodumene is concerned, we believe that the specimens from Norwich are the most beautiful that have been found anywhere, and we shall take pleasure on some early occasion in furnishing Rammelsberg with good specimens from both Norwich and Sterling.

36. *Petalite.*

In connection with the analyses of spodumene, it was thought interesting to examine the American petalite. For this purpose a specimen from Bolton, Mass. was selected. The results of analyses are,—

	1.	Oxygen.		2.	Oxygen.	
Silica,	77·95	40·50		77·90	40·47	
Alumina,	16·63	7·77	7·95	15·85	7·41	7·56
Peroxyd of iron,	0·62	0·18		0·51	0·15	
Lime,	*tr.*			*tr.*		
Magnesia,	0·21	0·08		0·26	0·10	
Lithia,	3·74	2·07	2·27	3·52	1·94	2·18
Soda,	0·48	0·12		0·53	0·14	
Potasa,	*tr.*			*tr.*		
Ignition,	0·60			0·70		
	100·23			99·37		

No. 1, gives oxygen ratio 20 : 3·92 : 1·12.
" 2, " " 20 : 3·74 : 1·08.

or quite nearly 20 : 4 : 1, from which we have the formula $\dot{R}^2\,\ddot{S}i^4 + 4\dot{A}l\,\ddot{S}i^4$ = Silica 78·37, alumina 17·44, lithia 3·40, soda 0·79.

Art. XXXIX.—1. *Action of Nitric Acid on the Chlorids of Potassium and Sodium.*—2. *Action of Oxalic Acid on the Nitrates and Chlorids of the same, with a ready method of converting them into the Carbonates.*—3. *The presence of Oxalic Acid enabling Zinc to decompose Water;* by Prof. J. Lawrence Smith, M.D., of Louisville, Ky.

This note is intended as an appendix to my researches for determining the alkalies in insoluble silicates in this Journal.*

During that investigation, many novel and interesting reactions were observed, several of which have already been alluded to. I present here one or two others of some interest.

* This volume, page 53.

It is well known that if nitric acid be added to a chlorid, or hydrochloric acid to a nitrate, more or less of a decomposition will in either case ensue; but I believe it is not generally known how ready and complete the replacement is, when nitric acid is heated with chlorid of potassium or of sodium.

Among the experiments made, 40 grammes of nitric acid were boiled gently with 6 grammes of chlorid of potassium, and in twenty minutes, no trace of chlorine could be found in the liquid; the same is true when the chlorid of sodium is used. The operations appear to depend on the oxydizing property of the nitric acid, with the liberation of chlorine that combines with some of the elements of nitric acid to form the chloronitric acid that readily passes off. The decomposition of the nitrates of the alkalies by hydrochloric acid does not readily takes place, it not being complete even after repeated evaporations to dryness, with a large excess of hydrochloric acid.

2. Before setling on the plan I now adopt, an easy method was sought for separating the alkalies from magnesia, by converting the two into carbonates,—a plan that had previously been adopted; but the question with me was to change the nitrates to carbonates. The idea suggested itself of heating the nitrates with an excess of oxalic acid, to the temperature at which the latter undergoes decomposition, when the nascent oxyd of carbon might break up the constitution of the nitric acid, and the carbonic acid formed combine with the bases.

On making the experiment, I was surprised to see an abundant evolution of nitrous acid vapors at a temperature considerably below 212°. It was clear that the oxalic acid decomposed the nitrate, liberating the nitric acid, which reacting on the excess of oxalic acid, gave rise to the nitrous acid vapors. If crystallized oxalic acid and the nitrate of potash or soda, the former in excess, be placed together in a flask, and heated over a water-bath, the mass soon enters into watery fusion, and at the temperature of from 130° to 140°, bubbles of gas are evolved, consisting of nitrous acid and carbonic acid; at 212° the evolution is vigorous, and if after evaporation to dryness the water be renewed several times, the nitric acid will be completely expelled from the nitre, there remaining the excess of oxalic acid, and the oxalate of the alkalies.

It was natural to conclude from the above result, that oxalic acid would likewise decompose the chlorids of the alkalies, and on experiment, the conclusion proved to be correct. If an excess of oxalic acid be mixed with either the chlorid of potassium or of sodium, and the whole warmed gently, abundant vapors of hydrochloric acid are evolved, and by careful manipulation, all the chlorine may be driven off under this form.

If heat be applied to the mass resulting from the action of oxalic acid on either the nitrates or the chlorids, all the oxalic acid will be expelled, and the oxalates converted into carbonates. A small amount of chlorid of sodium can in this way be converted in a few moments into carbonate of soda, not, however, without some trace of the chlorid being present. It is not my object to point to any special application of this decomposition, but it is one that may come into play in certain operations in analytical chemistry.

Experiments were made with the sulphate of the alkalies, to see if the oxalic acid had any decomposing action on them, expecting to test for free sulphuric acid, by the action of the solution of the mass on zinc or iron, taking for granted that the presence of oxalic acid alone would not cause the evolution of hydrogen gas. Experiment, however, showed that this manner of testing the question was fallacious, and no other method suggesting itself, it was impossible to decide the question positively; sufficient was ascertained to show that if the sulphate was decomposed, it was only to a very minute extent.

In connection with this last experiment, it is proved that zinc decomposes water readily in presence of oxalic acid, hydrogen gas being evolved; the action ceases in a short time, from the formation of insoluble oxalate of zinc; with iron, the action is very feeble even when the solution is heated.

The decomposing action of oxalic acid on the nitrates and chlorids of alkalies, appears to be due simply to the fact of a more stable acid being able to replace a more volatile one, and in no way measures the relative strengths of the acids; it being a well established fact that the physical as well as chemical properties of acids have much to do with their capability of replacing each other; a mere change of circumstances often reversing their relative action.

ART. XL.—*On the Blood-corpuscle-holding Cells, and their relation to the Spleen;* by W. I. BURNETT, M.D., Boston.

THE history of the spleen is a remarkable one in physiological science. From the earliest times to the present day, it has been the opprobium of investigators, and the dignified old Haller, after searching in vain for its functional relations, concluded that it was an unimportant and unworthy part in the economy. No less than fifteen theories as to its use have been advanced and defended up to the present time; these embrace every ostensible view of its function, having any ground of plausibility. It can be easily conceived, then, that modern physiologists might conclude, when at last from chemical and microscopical research, the true function of this

organ should be made out, that it would be only one of the old theories revived. But this, modern ingenuity has shown may not be the case; for, quite recently a theory has been advanced on this subject, which is unique and in no respect like any of the preceding. Kölliker, the distinguished Wurtzburg professor of anatomy and physiology, maintains that the spleen is the blood-destroying organ. It is indeed a significant question in physiology —what becomes of all the blood-corpuscles that have been used in the system? We find no traces of their decay in the blood-vessels, and, from all the appearances in the circulation, they never pass away. After they have served their part as oxygen-carriers, where is their final resting place? Kölliker supposes that this refuge is in the spleen, an organ in which they congregate, assume the form of globular masses, then crumble and are dissolved, passing off for a biliary or some other purpose.

This point opens for discussion several interesting topics, chief among which is the *modus operandi* by which Kölliker thinks this process is accomplished.

In order to understand this, the general relations and the intimate structure of the spleen must be briefly noticed.

None of the Invertebrata possess this organ: but it may be said, in general, to be present in all the Vertebrata, for its doubtful existence is with some of the Myxinoid fishes only, and even here it may well be questioned if the general rule does not hold good. It is evident, therefore, that the Vertebrata have relations in their economy on which the presence of this organ depends. This point, however, will be reserved for future discussion. In its gross aspect, the spleen appears as a red, pulpy organ, with white points here and there dotting its surface when its investing membrane is removed, or when its structure is exposed by an incision. This red, pulpy matter is composed chiefly of blood-vessels held in place by an intercellular or delicate parenchymatous substance made up of nucleated cells, and of peculiar muscular fibres which are of recent description.

The white points are the Malpighian corpuscles, which consist of vesicles filled with granular, nucleated cells not unlike those found in the general parenchyma. Such is the intimate structure of this organ with the higher Vertebrata; but with the lower Vertebrata, it is much less complex, no Malpighian bodies being found. On the whole, the spleen is a vascular, parenchymatous organ, composed so largely of blood-vessels that it has much variability of size in the same individual according to the flaccidity or turgidity of these vessels. We will now proceed to the points at issue in Kölliker's theory of the function of this organ.

In the splenic tissue of many animals, there are not unfrequently observed roundish or oval masses of a brownish color, and of a size varying from $\frac{1}{200}$ to $\frac{1}{1500}$ of an inch in diameter. These

appear to be aggregations or collections of blood-corpuscles which may or may not be invested with a distinct capsule. These corpuscles are in some stage of disintegration, and appear more or less brown, as this dissolution proceeds. These bodies, Kölliker thinks occur very generally and constantly in the spleen, and he supposes that, by them, this organ serves for the working up of old worn out blood-corpuscles.*

I need scarcely say that so novel a theory, and one, too, with such ostensible plausibility, has attracted the attention and provoked much discussion among physiologists. Indeed, I know of no special subject in physiology, that has received more attention from some of the best Microscopists, than this; Remak, Ecker, Virchow, Gerlach, Reichert, Kölliker, and many others, have given it special attention, and already its literature is not insignificant. I cannot here give an analysis of the somewhat different results of these observers, but it may be mentioned that although all admit that bodies such as I have described, do occur in the spleen, yet none of them, as I understand, accept Kölliker's hypothesis of their functional relations,† and all have not observed them with that frequency and constancy mentioned by Kölliker. As for myself, I have carefully examined the spleen throughout the Vertebrata, with special reference to the doctrine in question. These examinations have extended through whole tribes of fishes, reptiles, birds and mammals. I have not found these bodies by any means as common as Kölliker has stated; indeed they have appeared to me under circumstances which seemed more accidental than normal, and, with comparatively few exceptions, they were unenclosed by a distinct membrane. This was found to be the case especially with birds and reptiles. It is true that with the rabbit and other rodents which I have examined, these bodies were regularly saccular when present, but they were of various sizes; and it is worthy of notice that, in these animals, blood, taken from other parts of the body, showed a disposition to a grouping of its corpuscles into masses of variable size.

In some reptiles I have found these bodies present at one time and absent at another, and with the frog, where Kölliker says they may be beautifully seen, I have generally failed to detect them. On the whole, then, my conclusion is, that the blood-corpuscle-holding cells of Kölliker, are accidental rather than normal productions, and that they sustain no relations to the function of the spleen. I regard them as simple, minute extravasations of the blood into the spleen parenchyma, which may or may not be invested with a membrane according to the greater or less plasticity of the blood.

* This view Kölliker advanced in 1847, but it was first enunciated in a complete form in 1849, in the article *Spleen*, in the Cyclop. Anat. and Physiol. See literature.

† I would except Gerlach, Ecker, and Beclard; see literature, beyond.

This view is supported by many facts which have been observed in both physiology and pathology. If inflamed blood be drawn from the body and allowed to stand, there are, not unfrequently, found at the bottom of the vessel, bodies consisting apparently of little sacs filled with blood-corpuscles. This is due to the fact that a certain number of corpuscles become collected into one mass and this last is then invested by a membrane composed of the blood-plasma.

That this is the correct version is shown by the fact that these capsular bodies sometimes include other matters beside simple blood-corpuscles, and Kölliker himself mentions a case where, in an extravasation of blood into the *commissura mollis*, he found them containing pieces of cerebral substance. I would mention also, that I have observed these conditions distinctly marked in some blood of an elephant, which was recently examined.

My opinion, finally, is like that of Remak, that these peculiar bodies are minute blood-clots, (*Blutgerinnsel*) of accidental occurrence, and of no physiological significance.

It now remains for me to notice in this connection another subject, to which the one we have just treated is somewhat allied. I mean the so-called pigment-holding cells of Remak. The history and relations of these bodies have been brought out recently by the discussion of the constituents of the spleen to which we have just alluded. If the spleen of fishes is examined, there will be found on the sheath of some of its smaller arteries, roundish or oval bodies, which consist of a thick-walled capsule enclosing pigment granules. These are the bodies in question; they have not unfrequently been mistaken for the Malpighian corpuscles. They are tough, and the last parts of the spleen to decay, so that they are often best seen when the contiguous parts are just beginning to disintegrate.* I have never observed them in this connection with the minute arterial sheaths except in fishes. Here I have carefully studied them; they appear to be perfectly closed sacs, and have no communication with the artery on which they rest.

The constancy of their presence, as well as the general regularity of their size in individual species, would lead me to agree with Remak in considering them as true physiological organs. They seem to be producers of pigment granules, the ulterior use of which remains yet to be discovered. It may be that these granules are concerned in the coloring and special constituents of the bile.

The following appears to be the most probable view of the function of the spleen in the economy, as elucidated by the more modern microscopical studies.

* For figures of these bodies, see,—
Kölliker, Article *Spleen*, Cyclop. Anat. and Phys., 1849.—*Ecker. R. Wagner's* Icones physiol., Lfrg. I, 1851, Taf. vi.—*Remak*, *Müller's*, Arch. 1852, p. 115 Taf. v, fig. 4, 9, 10.

I think there can now be but little doubt that the systematic position of the spleen is in the category of ductless glands,—belonging, as to its physiological signification, with the Supra-renal capsules and the Thymus and Thyroid bodies. Its structure and its development, both, indicate the correctness of this view.

Experiments have shown that, whatever may be its function, the presence of the spleen is far from being indispensable in the body or even necessary for its health.* The whole history of its formation and development in the embryo, clearly shows that it can have no particular function during the embryonic life. In the chick, where its earliest conditions have been most carefully traced, it is found that it reaches no functional capacity,† until the period of incubation is over.

Its relations to the economy then do not begin until the animal has commenced to have a nutrition of its own. In this respect, therefore, it is inverse to the other ductless glands; for as has been well shown by Ecker and other observers, the Thymus and Thyroid bodies, and the Supra-renal capsules, are truly embryonic organs, and cease to perform any function in the individual animal.

I would, therefore, say that the whole tenor of present research upon this organ, combined with many incidental pathological phenomena, favor the view that the spleen has functional relations in the adult life, corresponding to those of the other ductless glands, just mentioned, in the embryo; that is, it is intimately connected with the formation of the red-corpuscles of the blood. I would not say with Gerlach and Schäffner‡ that it is the locality where these corpuscles are formed,—but rather that it has more the office of a lymphatic gland where the plastic materials of the blood are prepared and made ready for their conversion into the red-corpuscles; or, to speak more to the point, it is one of the organs in which chyle-corpuscles are formed and eliminated preparatory to their being changed into, or rather serving as the basis for the formation of, the true red-corpuscles. The Malpighian vesicles are undoubtedly prominent agents in these processes, and Gray has shown that their development here is precisely like that of the vesicles in the Thymus and Thyroid glands. In this connection should also be mentioned the fact of the existence of the spleen in all the vertebrates which have both red and white corpuscles in their blood, and its non-existence with all the Invertebrata which have the white corpuscles alone. Moreover, the spleen is most prominent in those vertebrates which have the red constituents of the blood most marked, while with the Myxi-

* The sensible remark of Haller is well worthy of quotation in this connection "In utilem aliquam partem corporis animalis esse, tam laté per divisas species regnantem, indignum est dictu."—Elem. physiolog. vi. 426.

† See *Gray*, "On the development of the ductless glands in the chick." Philos. Trans. 1852, part ii, p. 295.

‡ See literature, beyond.

noid fishes, whose blood quite resembles that of the Articulata, the spleen is so feebly developed that its existence was for a long time denied.

These data are worthy of remembrance, and without a more detailed discussion, I think we are justified in concluding, if any conclusion is now proper, that *the spleen is a vascular-lymphatic gland, whose function is intimately connected with the formation of the blood, by the elaboration of chyle-products.*

Literature of the Blood-corpuscle-holding Cells.

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Art. XLI.—*Extraordinary Fishes from California, constituting a new Family, described* by L. Agassiz.

About fifteen months ago, I received a letter from A. C. Jackson, Esq., soon after his return from San Francisco, California, informing me that while fishing in San Salita Bay, he had caught with the hook and line, a fish of the perch family, *containing living young*. The statement seemed so extraordinary, that though an outline of the specimen observed was enclosed, I suspected some mistake, and requested Mr. Jackson to furnish me further information upon what he had actually seen, and if possible specimens of the fish preserved in alcohol. To this enquiry, I received the following answer:

"I regret much that the information which I sent you avails so little, without the actual specimens of the fish and young; these however, I have already taken active measures to obtain, and trust before many months to be able to send you at least specimens of the female, if not of the young. I should at the time I caught the fish have preserved them in alcohol, but at that time I was attached to the Navy Yard commission, and was with my comrades industriously prosecuting the examination of the vicinity of San Salita, as to its adaptiveness for a navy yard, and could not leave for San Francisco without suspending the work, and the means for preserving the fish could not be otherwise procured. This explains the apparent culpable indifference which allowed me to omit preserving the specimens. I have sent directions to California to have caught for me several of the fish, and if at the present time (September 16th, 1852) the females were pregnant (which is not probable) to take from one the bag containing the young, and put mother and young in the jar of alcohol, and to put several other females untouched, into the jar also. These specimens will by direction and examination even if they be not pregnant, and if the jar contains no young, determine the truth and accuracy of the statement I made in my former letter on the subject. This fact proved by these specimens, it will be very easy to obtain during the next spring and summer, specimens in all stages of pregnancy. I think, if I remain in the country, I can insure you a sufficiency of specimens, to determine to your satisfaction, the true state of the affair, during the course of the next year. The fish I refer to, in my opinion, does not exist in very great numbers even in the waters of San Salita Bay, for the two which I caught on this occasion were the only ones which I fell in with, though I fished in the same place probably four times. There was a little peculiarity perhaps in the circumstance of my taking them as I did. I had previous to this time, tried my rod and line, as I mentioned before, four times, always with success as regards groupers, perch, &c., without a sight of the singular fish under consideration. A few days, perhaps a week, after the four trials, and on the *7th of June*, I rose early in the morning for the purpose of taking a mess of fish for breakfast, pulled to the usual place, baited with crabs, and commenced fishing, the wind blowing too strong for profitable angling; nevertheless on the first and second casts, I fastened the two fishes, male and female, that I write about, and such were their liveliness and strength, that they endangered my slight trout rod. I however succeeded in bagging both, though in half an hours subsequent work I got not even a nibble from either this or any other species of fish. I determined to change the bait, to put upon my hook a portion of the fish already caught, and cut for that purpose into the largest of the two fish caught. I intended to take a piece

from the thin part of the belly, when what was my surprise to see coming from the opening thus made, *a small live fish.* This I at first supposed to be prey which this fish had swallowed, but on further opening the fish, I was vastly astonished to find next to the back of the fish and slightly attached to it, *a long very light violet bag, so clear and so transparent, that I could already distinguish through it the shape, color and formation of a multitude of small fish (all fac-similes of each other) with which it was well filled.* I took it on board (we were occupying a small vessel which we had purchased for surveying purposes,) when I opened the bag, I took therefrom *eighteen* more of the young fish, precisely like in size, shape, and color, the first I had accidentally extracted. *The mother was very large round her centre, and of a very dark brown color, approaching about the back and on the fins a black color, and a remarkably vigorous fish.* The young which I took from her were in shape, save as to rotundity, perfect miniatures of the mother, formed like her, and of the same general proportions, except that the old one was (probably owing to her pregnancy) much broader and wider between the top of the dorsal and the ventral fins, in proportion to her length than the young were. *As to color they were in all respects like the mother, though the shades were many degrees lighter.* Indeed, they were in all respects like their mother and like each other, the same peculiar mouth, the same position and shape of the fins, and the same eyes and gills, and there can not remain in the mind of any one who sees the fish in the same state that I did, a single doubt that these young were the offspring of the fish from whose body I took them, and *that this species of fish gives birth to her young alive and perfectly formed, and adapted to seeking its own livelihood in the water. The number of young in the bag was nineteen,* (I fear I mistated the number in my former letter,) *and every one as brisk and lively and as much at home in a bucket of salt water, as if they had been for months accustomed to the water.* The male fish that was caught was not quite as large as the female, either in length or circumference, and altogether a more slim fish. I think we may reasonably expect to receive the specimens by the first of December. But I can hardly hope to get satisfactory specimens of the fish as I found it with young well grown, before the return of the same season, viz., June. By that time I trust the facts will be fully decided, and the results, as important as they may be, fully appreciated."

In a subsequent letter, (dated January 31, 1853,) Mr. Jackson informed me that he had requested Capt. Case, U. S. N., who commanded a sloop of war in San Francisco, and who had also seen the fish, to supply my friend T. G. Cary, Junr., Esq., of San Francisco, with specimens of that fish, should he succeed in getting any. I wrote myself also to Mr. Cary, to be on the look out for this fish.

About a fortnight ago, I was informed by Mr. Cary, in a letter dated San Francisco, August 10, 1853, that after a search of several months he had at last succeeded in obtaining several specimens of this remarkable fish, three of which were sent by express, (which have reached me lately), while a larger supply was shipped round Cape Horn. After a careful examination of the specimens, I have satisfied myself of the complete accuracy of every statement contained in Mr. Jackson's letter of February, 1852, and I have since had the pleasure of ascertaining that there are two very distinct species of this remarkable type of fishes, among the specimens forwarded to me by Mr. Cary. I propose for them the generic name of Embiotoca, in allusion to its very peculiar mode of reproduction.

I feel some hesitation in assigning a family name to this type. It is probable that all its members will present the same peculiarity in their mode of reproduction, and that therefore the name *Embiotoca* may with perfect propriety be modified into *Embiotocoidæ*, as Didelphis has given its name to a numerous family, the *Didelphyidæ*, after having been for a long time simply a generic name. Should it however be found that other types of this family present various modifications in their vivaporous reproduction, for which the name *Embiotocoidæ* might be objectionable, I would propose to frame some family name from another structural peculiarity of these fishes, not yet observed in any others, the naked furrow-like space parallel to the base of the posterior dorsal fin, separating the scales which cover the base of the rays, from those of the sides of the body and name it *Holconoti*.

The perseverance and attention with which Messrs. Jackson and Cary have for a considerable length of time been watching every opportunity to obtain the necessary materials for a scientific examination of these wonderful fishes, has induced me to commemorate the service they have thus rendered to zoology by inscribing with their names the two species now in my hands, and which may be seen in my museum in Cambridge, labelled *Emb. Jacksoni* and *Emb. Caryi*..

A country which furnishes such novelties in our days, bids fair to enrich science with many other unexpected facts, and what is emphatically true of California, is in some measure equally true of all our waters. This ought to stimulate to renewed exertions not only our naturalists, but all the lovers of nature and of science in this country.

Family *Holconoti* or *Embiotocoidæ*.

The general appearance of the fishes upon which this family is founded, is that of our larger species of Pomotis, or rather that of the broader types of Sparoids. Their body is compressed, oval, covered with scales of medium size. The scales are cycloid, in

which respect they differ widely from those fishes they resemble most in external appearance. The opercular pieces are without spines or serratures. Branchiostegal rays six. The mouth is encircled by rather thick lips; the intermaxillaries forming by themselves the whole margin of the upper jaw. The intermaxillaries and upper maxillaries are slightly protractile. Teeth only upon the intermaxillaries, lower maxillaries and pharyngeals; none either upon the palatines or the vomer. In this respect, as well as in the absence of spines and serratures upon the opercular pieces, they differ much more from the Percoids, than from the Sparoids; but the cycloid scales remove them at once from the latter, in which the scales present a very uniform ctenoid type. The thick lips might remind one of the Labroids, but the scales of the Embiotoca are neither elongated, nor provided with the characteristic branching tubes of that family.

One long dorsal fin, the anterior portion of which is supported by spinous rays, and the posterior by numerous articulated branching rays, which are sheathed at the base by two or three rows of scales, *separated from those of the body by a rather broad and deep scaleless furrow*. This last peculiarity has not yet been observed in any fish, as far as I know. There is indeed a distinct longitudinal space parallel to the soft portion of the dorsal, nearly of the width of a single row of scales, which is entirely naked and well defined, forming as it were, a furrow between the scales of the back, and those which rest against the base of the fin rays. Though protected in this way by a kind of sheath, the anterior part of the dorsal fin alone can be folded backwards and entirely concealed between these scales, as in many Sparoids; the posterior part only partially so. Moreover, the scales of the sheath are separated by a furrow from those of the back, only along the base of the soft part of the dorsal fin. The first rays of the anal fin are short, comparatively small and spinous. The base of this fin is strangely arched, and sheathed between scales, in the same manner as the dorsal; the spinous rays when folded back being more fully concealed in the sheath than the soft rays.

The ventrals are subthoracic as in the Sparoids, and provided with a strong spinous and five soft rays.

Four branchial arches, supporting four complete branchiæ with two rows of lamellæ in each. The opening behind the last arch is very small and entirely above the base of the pectoral fins. Pseudobranchia very large, and composed of sixteen or seventeen lamellæ. The alimentary canal is remarkably uniform in width for its whole length. It extends first on the left side as far back as the ventrals, turns forwards and upwards to the right, then follows the middle line along the *large air bladder*, to the second third of the abdominal cavity, then bends along the right side downward and slightly forwards almost to meet the first bend, when it

turns backwards again, and ends in a straight course at the anus. The stomach can not at all be distinguished externally from the small intestine by its size and form. There are no *cæcal appendages at all* in any part of the intestine. The whole alimentary canal contained large numbers of shell fragments of small Mytili. The liver has two lobes, a short one on the left side, and a long one along the middle line of the body.

The female genital apparatus, in the state of pregnancy, consists of a large bag, the appearance of which in the living animal has been described by Mr. Jackson; upon the surface of it large vascular ramifications are seen, and it is subdivided internally into a number of distinct pouches, opening by wide slits into the lower part of the sack. This sack seems to be nothing but the widened lower end of the ovary, and the pouches within it to be formed by the folds of the ovary itself. In each of these pouches a young is wrapped up as in a sheet, and all are packed in the most economical manner as far as saving space is concerned, some having their head turned forwards, and others backwards. *This is therefore a normal ovarian gestation.* The external genital opening is situated behind the anus, upon the summit and in the centre of a conical protuberance formed by a powerful sphincter, kept in its place by two strong transverse muscles attached to the abdominal walls. The number of young contained in this sack seems to vary. Mr. Jackson counted nineteen; I have seen only eight or nine in the specimens sent by Mr. Cary, but since these were open when received, it is possible that some had been taken out. However, their size is most remarkable in proportion to the mother. In a specimen of Emb. Jacksoni, $10\frac{1}{2}$ inches long, and $4\frac{1}{2}$ high, the young were nearly three inches long and one inch high; and in an Emb. Caryi, eight inches long, and $3\frac{1}{4}$ high, the young were $2\frac{3}{4}$ inches long, and $\frac{7}{8}$ths of an inch high. Judging from their size, I suspected for sometime that the young could move in and out of this sack like young opossums, but on carefully examining the position of the young in the pouches, and also the contracted condition of the sphincter at the external orifice of the sexual organs, I remained satisfied that this could not be the case, and that the young Mr. Jackson found so lively after putting them in a bucket of salt water, had then for the first time come into free contact with the element in which they were soon to live; but at the same time, it can hardly be doubted that the water penetrates into the marsupial sack, since these young have fully developed gills. The size of the young compared with that of the mother is very remarkable, being full one-third its length in the one, and nearly so in the other species. Indeed these young Embiotocæ, not yet hatched, are three or four times larger than the young of a Pomotis (of the same size) a full year old. In this respect these fishes

differ from all the other viviparous species known to us. There is another feature about them of considerable interest, that while the two adult differ markedly in coloration, the young have the same dress, light yellowish olive with deeper and brighter transverse bands, something like the young trouts and salmons in their Parr dress. The transversely banded species may therefore be considered as inferior to the other, since it preserves through life the system of coloration of the embryo.

It will be a matter of deep interest to trace the early stages of growth of these fishes, to examine the structure of the ovary and the eggs before fecundation takes place, etc., etc. The state of preservation of the specimens in my hands, precluded every such investigation.

Though I know thus far only one single genus of this type, I do not think it right to combine the generic characters with those of the family, as is generally done in such cases, as I would also object to the practice of omitting any specific characteristics where only one species is known of a genus. This shows an entire misapprehension of the relative value and subordination of the characters of animals. I would therefore characterize as follows the genus

Embiotoca, Agass.

Body much compressed and elevated. Head small, with scales only on the cheeks and opercular pieces. Teeth in both jaws, short, conical, arranged in one row, and slightly recurved. The pharyngeal teeth much shorter and blunter than those of the jaws, and arranged like pavement. Dorsal fin with nine or more spinous rays. The first three rays of the anal fin, spinous, and much shorter than the following articulated rays, which are always finer and more numerous than the corresponding rays of the dorsal fin. The lateral line is continuous to the base of the caudal fin. Whether the peculiar mode of reproduction is a family or a generic character, remains to be ascertained by further investigations. It is however probable that with some slight modifications it will be found the same in all the members of the family.

Some differences between the two species observed, might render it doubtful whether they ought to be considered as belonging to as many distinct genera or not. But we know that in genera differing greatly from others, the range of the specific differences is also wider than in genera with many species; so until I am taught differently by new discoveries, I would refer them both to one and the same genus. Such doubts could scarcely be entertained respecting families with many genera, where a standard to estimate genuine generic differences is easily obtained.

1. *Embiotoca Jacksoni*, Agass.

The body is quite high, of an oval form, greatly compressed and similarly arched above and below. The superior arch extends to the posterior base of the dorsal fin, whence it continues in a horizontal line to the base of the tail. The ventral arch of the body is similar to that of the dorsal outline. The profile from the dorsal fin to the end of the snout, is rather precipitate and regularly arched, except obliquely above and in front of the eyes, where it is slightly concave. The greatest height of the body, including the dorsal fin, is equal to the distance from the end of the snout to the extremity of the pectoral. The greatest thickness of the body is equal to one-fourth its height. The head is of moderate size, its length, measuring to the posterior angle of the opercle, being about one-fourth that of the entire fish. The mouth is quite small, the hind extremities of the intermaxillaries and maxillaries extending not farther back than the line of the anterior border of the orbit. But a small portion of the superior maxillary is exposed at the angle of the mouth. The anterior edge of that part of the snout into which the intermaxillaries fit, is on a horizontal line drawn immediately below the orbits. The upper jaw is slightly more prominent than the lower, the teeth of the latter fitting *within* those of the former. In the upper jaw there are fourteen or fifteen teeth; in the lower there are two or three less. They all are slightly swollen near the top, and are not pointed but rather bluntly edged. They do not extend to the angles of the mouth, but leave a space without teeth on each jaw. The teeth of the upper jaw are but little larger than those of the lower. The teeth of the pharyngeals are much shorter than those of the jaws, and form two quite moveable plates above, and a triangular one below. There are not more than thirty teeth on each of the superior plates, and mostly truncated at the top. The four or five teeth, which form the inner row of each plate, are more prominent than the others, and somewhat pointed. The teeth of the inferior pharyngeal plate are similar to those of the upper, but the teeth of its posterior range are the most prominent, and pointed. The lips are rather fleshy, and entirely conceal the teeth. Beneath the lower lip there is an elongated pit on each side, extending towards the corners of the mouth; it is covered by a thin border of the lip. The distance from the end of the snout to the anterior border of the orbit, is greater than the diameter of the latter by one-third. The inferior margin of the orbit is on the middle longitudinal line of the body; and its posterior border is half way between the end of the snout, and the posterior angle of the opercle. The opercular pieces are large. On the preopercle are four concentric rows of scales; the two inner and anterior are the longer. There are thirteen large scales in the row nearest the eye, and the number is

less and less in the others. Still within the row nearest the eye, there is a space without scales, and marked by pores radiating from the edge of the orbit. The posterior and inferior border of the preopercle, outside of the ridge of the latter, is thin, membranous, and without scales, but marked with numerous pores or tubes similar to those around the orbits, and radiating from within outwards.

The opercle, subopercle, and interopercle are covered with scales, which decrease in size from the former to the latter. There is a narrow membranous border to the opercle, extending from its posterior angle to the height of the termination of the lateral line. The notch between the subopercle and interopercle is on a vertical line with the edge of the posterior border of the preopercle. There is a small patch of scales, nine or ten in number, immediately above the superior attachment of the preopercle. The dorsal fin extends over about $\frac{3}{5}$ths of the superior curve of the body; its posterior portion is one-third higher, as well as longer, than its anterior. The spinous portion has nine or ten rays, the length of the first of which, is equal to one-third that of the last. At the point of each spine, the fin appears to extend backwards in a loose filament. There are $19\frac{2}{1}$ articulated rays in the dorsal fin: the superior outline of this part is nearly similar to that of the back, although the rays of its first half are the longest, and nearly equal in length. The furrow on each side extends as far forwards as the base of the first articulated ray, where there are three rows of scales forming the sheath; but the rows are reduced to one towards the posterior attachment of the fin.

The pectoral fins are of rather large size, and are placed below the middle line of the body, as well as below the posterior angle of the opercle. They extend about as near to the anal fin, as do the ventrals. The second ray of the pectoral is but slightly arched towards its extremity. There are twenty-one rays in each pectoral. The base of the ventrals is just in advance of the middle of this second ray of the pectoral. The spinous ray of the ventrals is $\frac{3}{5}$ths the length of the following articulated ray. There is a long plate of scales between the ventrals. The anal fin is broad and composed principally of fine slender rays. The last and longest of its spinous rays, equals in length $\frac{1}{4}$th that of the following articulated ray, which latter is equal to the corresponding ray of the dorsal fin. The last ray of the anal fin is placed nearer the caudal fin than that of the dorsal. The fin itself reaches nearer the base of the tail. The caudal fin is deeply forked; it contains fourteen rays, omitting its outer and short rays. There are eight rows of scales between the lateral line and the spinous portion of the dorsal fin, and eighteen rows below the lateral line in the same region. Sixty scales in the lateral line. Color uniformly dark olive brown, along the back, fading slightly upon the sides; dor-

sal black, mottled with white; caudal blackish, lighter upon the base; anal deep black, with a light longitudinal band; pectorals white; ventrals black with light base.

From the above description, it must be obvious that this is the species first observed by Mr. A. C. Jackson, to whom I have inscribed it, or at least a species very closely allied to it. There is only one fact about it which surprises me, that while he observed mature young in it on the 7th of June, Mr. T. G. Cary should have found it still with young as late as the beginning of August. Again Mr. Jackson saw nineteen young in it, whilst in the specimens forwarded by Mr. Cary, I found only eight or nine young, which were transversely banded like Emb. Caryi. May there be two species so closely allied as to be easily mistaken? I must add, that Mr. Jackson does not mention the mottled appearance of the dorsal, nor the light band upon the anal of his fish; which renders the supposition more probable that there are several and not only two species of this remarkable genus, about San Francisco. I would however, not forego the opportunity of connecting the name of Mr. Jackson with his interesting discovery, and have therefore called *Emb. Jacksoni*, that one of the species sent me by Mr. Cary, which agrees most closely with his description, leaving it for the future to decide whether this species is truly the one he first saw, a circumstance which is quite immaterial, since we already know two species of this extraordinary type.

2. *Embiotoca Caryi*, Agass.

The body is much more elongated than in Embiotoca Jacksoni yet equally compressed. Its height, including that of the dorsal fin, is less than the distance from the end of the snout to the extremity of the pectoral; and less than one-half the length of the fish. The profile is much less steep, and the snout quite as prominent, hence the head is longer than high. The posterior border of the orbit is nearer the angle of the opercle than the end of the snout. The upper and lower curves of the body are equal, and approach more nearly towards the tail, making this latter narrower than in the first species. The scales of the back do not descend upon the head lower than one-half the distance from the first spine of the dorsal to the end of the snout. The forehead is slightly concave as in Emb. Jacksoni. The posterior end of the intermaxillary does not extend as far back as the anterior border of the orbit. The nature of the lips, and extent of the upper maxillary is much as in the other species, but the anterior edge of the socket of the intermaxillaries is *above* the line of the lower border of the orbit. A vertical line through the orbit shows the height of the head in this region to be one-third less than in E. Jacksoni. The opening of the mouth is directed more obliquely

upwards. The teeth are more slender, but have otherwise the same form. In the upper jaw there are twelve, in the lower eight teeth. The nasal openings are of tolerable size; one before the other, and in advance of the eye, but slightly below the line of its superior border. The vertical diameter of the orbit is less than its longitudinal; and its posterior border is nearer the angle of the opercle than the snout. The preopercle in this species is less rectangular than in the former. The inferior rounded angle of its *ridge* is in *advance* of the posterior margin of the orbit. The scales of the preopercle are also much smaller and less conspicuous. Tubes radiate from the border of the orbit and from the ridge of the preopercle, as in Emb. Jacksoni. The posterior membranous border of the opercle is narrower: the notch between the subopercle and interopercle is on the vertical line of the posterior border of the preopercle. There is a patch of scales above the superior attachment of the preopercle. The dorsal fin differs very little in form from that of the former, but extends somewhat farther forwards, its first spine being immediately over the posterior angle of the opercle. The distance from this spine to the end of the snout equals the distance from the same back to the ninth articulated ray. The posterior rays of the articulated portion, are shorter than in the first species, but they are more numerous by three rays. The pectoral has twenty-one rays; it is perhaps longer than in the other. The ventrals differ little. The anal fin however, differs greatly: it is very small and contracted, and is placed far behind the ventrals. The scales at its base form a waved outline much more marked than in E. Jacksoni. The spinous rays are very short, the last being less than one half the length of the following articulated ray, the base of which latter is directly under that of the fifteenth corresponding ray of the dorsal fin. Its posterior base and termination are as in the first species. The caudal fin however, is more slender, and more deeply notched. The scales of the body are by no means so large. The lateral line, follows the outline of the back, as in E. Jacksoni; there are seventy-five scales in it.

Color light olive, darker along the back; light brown longitudinal bands extend between the rows of scales, and darker transverse bands reach from the back to the sides of the body, not extending below the lateral line in the anterior part of the trunk, but more marked, and reaching nearly to the anal fin upon the tail. Head mottled black and white. Dorsal and caudal dotted with black and white. Anal with a large diffuse black mark upon lighter ground. Pectorals white. Ventrals white at the base, terminated with black.

Only one female has been observed, containing eight young. This species was discovered by T. G. Cary, Esq., in the Bay of San Francisco, in the beginning of August 1853.

ART. XLII.—*On a change of Ocean Temperature that would attend a change in the level of the African and South American Continents;* by JAMES D. DANA.

THE idea of a change of climate consequent upon a change in the distribution of land and water on the globe, brought forward by Sir Charles Lyell, has recently been discussed with much ability and precision, by Prof. Hopkins, especially with reference to the Northern Atlantic. As there is profit in this consideration of possibilities whether we can prove the actual occurrence of the supposed events or not, we briefly remark in this place upon another geological change that would affect the temperatures of both the Pacific and Atlantic Oceans.

Upon the oceanic isothermal chart issued with the last number of this Journal, and discussed in that and this number, it is observed that the whole western coast of South America is bordered by cold waters; and that while in the Pacific, 80° F. is the coldest temperature of the year in mid-ocean, towards South America, even under the equator, the ocean temperature of 74° is not found, in the cold season, short of a distance of 2500 miles from the coast.

We have also remarked upon the evidence that a similar southern or extratropical current affects the temperature of the whole southern Atlantic, (see page 320) and makes this literally the cold ocean of the globe.

It is moreover evident from the temperature of the waters off western South America, that the extratropical or antarctic current has a vastly wider influence here than in the southern Atlantic; the positions of the lines of 68° and 74° in the two regions make this sufficiently apparent. It is also obvious, that the South American Continent, by extending so far south,—22 degrees, or 1300 miles, beyond the south point of Africa,—should necessarily intercept to a large extent the antarctic current, and thus occasion in connection with other causes, the northern flow that influences so widely the temperature of the waters off this coast. The position of the isocryme of 35°, shows that this same current flows on, rising somewhat northward towards Cape of Good Hope; yet the African continent lies so far to the north, that it can in fact intercept but a small part of the southern current, which consequently to a large extent passes on south of the Cape; yet this small part produces the wonderful effects pointed out.*

* We find that at the recent meeting of the British Association, Mr. A. G. Findlay, in the course of a paper on the oceanic currents of the Atlantic and Pacific, takes the common view that the Lagulhas current is the origin of the current that flows up the West African coast, a view shown on page 322 to be untenable.

Suppose now, that by a change of level, America were to terminate in latitude 34° S., and Africa in latitude 56° S.: the relation of the two, and of the cold influences of the currents adjoining, would be entirely changed. The vast area in the South Pacific, embraced between the west South American coast and the isocryme of 74°,—which marks the influence in the colder season of the cold southern waters, though not by any means its extreme limit,—would, if transferred to the Atlantic equatorial regions, stretch nearly or quite across from Guinea to the East Cape of South America; and the line of 68° would sweep around north of the equator quite to mid-ocean. The actual extent of the change may be perceived with close accuracy if we transfer the isocrymal lines off this part of Western America to the Atlantic. In the Pacific, under the same circumstances, the line of 68° would nowhere reach within several degrees of the equator.

The distribution of marine life would be greatly changed. While now the west coast of South America is, as regards the ocean, one of the coldest regions for the latitude in the world, it would become very much moderated, and a considerable portion of coast would be bordered by tropical waters. Along by Lima, and far south, there might be coral reefs. In the Atlantic, on the contrary, the Gulf of Guinea now characterized by torrid waters, would be filled with the colder seas of the temperate zone, and true tropical life would be altogether excluded.

The influence also on the Gulf Stream would be very decided and the whole North Atlantic would feel the change.

It is a remarkable fact that while the west coast of America is bordered in the tropical part by cold waters, 10° to 12° below the mean of mid-ocean, and the marine zoology is hence extratropical, the temperature of the land is peculiarly torrid over the same latitudes. It is evident that in judging of the influence of the ocean temperature on the temperature of the land, the direction of the aerial currents for the year, should be considered as a most important element towards any just conclusions.

Although we cannot show that the supposed change of level in the continents has taken place, we may learn from the facts what vast changes in marine life, have happened in past ages, through such changes of level as have occurred in the earth's history. The changes on the land from this cause would be less marked; besides, these have had far less influence on the life of the rocks than those of the ocean, as the fossiliferous rocks are mainly of marine origin. We know that in the cretaceous and tertiary periods, the Andes were in part under water, or at a much lower level, and effects of the kind considered, cannot be altogether hypothetical.

Art. XLIII.—*Reviews and Records in Anatomy and Physiology;* by Waldo I. Burnett.

I. (1.) *Theorie der Befruchtung und über die Rolle, welche die Spermatozöiden dabei spielen;* von Dr. Th. Ludw. Wilhelm Bischoff, —in *Müller's* Arch. 1847. p. 422–443.

(2.) *On the Impregnation of the Ovum in the Amphibia, and the direct agency of the Spermatozoon;* (1st Series.) Philosophical Transactions, 1851, p. 169; (2nd Series revised.) Proceed. Royal Society, June 17, 1852; by George Newport, F.R.S., &c., &c.

(3.) *De Spermatozoorum introitu in ovula. Additamenta ad Physiologiam Generationis.* Auctore Gotth. Aug. Ferd. Keber, &c. Accedunt lxxxi figurae chalcotypicæ textui insertæ et in iv Tabulas collectæ. 4to, pp. 118. *Regiomonti Prussorum,* mdccclIII.

With every inquiring mind there is a deep interest connected with the development of animal life. To watch the origin and rise of new forms, to trace the successive phases through which they pass, as the ideas on which they are based become more and more definitely expressed, until finally the perfect animal is produced,—these have been favorite studies from the earliest times with some of the most genial minds, and over which they were accustomed to dwell with increasing delight. But more interesting still, because more wonderful, is the study of those necessary preliminaries of all individual development—the mysterious conditions of fecundation. To observe, after nature has prepared the material, how she puts up a new structure and to trace the adaptive idea in the laying of each part, require but opportunity united with careful diligence and patience. But to lift the veil beneath which lie hidden the more than mysterious relations of individuality, this is to tread on the confines which separate the material from the immaterial world.

There is no question in physiology so difficult and at the same time so interesting as—*How* is a new individuality started by the conjugation of the sexes; and where so little could be observed, there has been more scope for speculation.

In modern times, however, with certainly better instruments if not better opportunities, we have looked for less talk and more knowledge; and in this respect, it may be justly said that we have approached pretty near that boundary, which, as it is the limitation of that which can be perceived by the senses, is the real confine between the known and the unknown in physical science.

We have selected the above-cited works, because they comprise some of the most important recent contributions upon a subject we here propose to take up in a somewhat discursive manner. As it would be profitless to notice the labors of those numerous men,

who, in this department have written upon what they really knew nothing, yet speculated much, we shall attempt to show the state of our real knowledge on this *ultimatissimum* of physiology —the *modus operandi* of fecundation.

Modern histological studies, have we think, pretty definitely settled two fundamental and important points: 1st. That the ovum is, morphologically, only a nucleolated cell; and 2nd. That the sperm-cell is the true homologue of the ovum.

The ovum (fecundated) produces the embryo; the sperm-cell the spermatic particle. The embryo and the spermatic particle are the correlative representatives of the female and the male sex. One is the metamorphosed nucleus (vitellus) of the one; the other the metamorphosed nucleus (nucleus of the daughter-cell) of the other. In both, the ovum and sperm-cell, the process of segmentation seems a necessary preliminary to the evolution of the new being.*

The strict correlation between the essential products of the sexes is as wonderful as it is beautifully suggestive of the unity and simplicity of plan by which nature proceeds. This point, so seductive in all its relations, might be dwelt upon in detail, but we will continue with main and general facts. The ovum, as a nucleated or nucleolated cell, continues to grow, and whatever size it may attain to by the endogenous formation within its capsule of new cells, yet, when complete, it is, (even though belonging to the *Ostrich* or *Epiornis*,) morphologically, only a great compound nucleated or nucleolated cell. All these conditions of origin, growth and maturity, can be satisfactorily studied in the lower animals, and we would especially recommend the compound Ascidiæ for this purpose. The ovum, thus complete, is ready for fecundation.

We have already said that the sperm-cell is the analogue, or more properly *homologue*, of the ovum; its origin and development, as we have traced them in all their details, are precisely the same as those of the ovum. The sperm-cell increases to a definite size, its nucleus (vitellus) then regularly segments 2, 4, 8, 16, &c., and the results of this segmentation, are daughter-cells. The condition of the sperm-cell at this moment is like that of the ovum produced by the same process of segmentation. I mean the *mulberry-like* condition. But at this point there is a digression, for with the sperm-cell the nucleus of each of the daughter-cells is changed into a spermatic particle, while with the ovum, the whole mass is metamorphosed into the new being by a process of substitution.

The spermatic particle, then, is only a metamorphosed nucleus of a cell, and, perhaps, were the analogy carried out completely, each daughter-cell would be the representative of a miniature ovum.

* See Researches on the origin, mode of development, and nature of the spermatic particles among the four classes of the Vertebrata.—*Mem. Amer. Acad., N. S.*, v, *pt.* 1, 1853.

Physiologically, the phenomena we have thus briefly described, obtain equally in the vegetable kingdom; for, as recent discoveries have shown, even in the simplest cellular plants there is a conjugation of two kinds of cells, the product of which terminates in a new generation; and in the other plants, the superior Cryptogamia, and the Phanerogamia, there are parts which in a developmental as well as a morphological point of view, correspond to the essential male and female products of animals.*

Throughout the organized world, therefore, the conditions which wait upon the true generative process are the same—the combination of the representative products of two distinct sexes—and these products, whatever may be said of their form, are always physiologically the same;—they are cells or cell-products.

Here we would make a general statement which embodies a great deal of physiology on this subject: A true generation must be regarded as resulting only from the conjugation of two opposite sexes, from a sexual process in which the potential representatives of two individuals are united for the elimination of one germ. The germ power thus produced may be extended by gemmation or by fission, but it can be formed only by the act of generation, and its play of extension and prolongation by budding or by division must always be within a certain cycle, and this cycle is recommenced by the act of the new conjugation of the sexes.

In this discussion, we have satisfactorily reached this point that the ovum and the spermatic particle are the potential representatives of the sexes to which they respectively belong. From their union results the condition of fecundation; the grand question now is, what is the *modus operandi* of this fecundating act? Here we touch upon debatable ground, and the three authors with which we have headed these remarks are the advocates of as many dissimilar views. Bischoff's view, based upon speculative probabilities rather than upon observation, is, that contact alone of the spermatic particle with the ovum being sufficient for fecundation, impregnation consists in a kind of catalysis which has its exemplification in chemical conditions as enunciated by Liebig, (see loc. cit., p. 425.) But if catalysis embodies conditions in the organic like those of its relations in the inorganic world, it falls very far short of affording the requisite explanation of these phenomena, as we hope soon to show. This field of probabilities and possibilities we shall enter upon again.

Newport's contributions upon the physical phenomena of this subject are far the most complete that we have, and being the results of a most trustworthy observer, they deserve our special attention. That his views may be the more clearly understood,

* We would refer to a profoundly physiological memoir by *Robin*, titled: "Ovum, its existence as well in the male as in the female of plants and animals," &c. Compt. Rend., 1849.

we quote his own language, as used in some of the conclusions which he has given. We will commence with the 5th, omitting the preceding four as irrelevant to our point in question:

"5th. That only extremely minute granules of solid matter can by any possibility pass into the tissue of the envelopes during endosmosis; and that there is no evidence whatever of the existence of a fissure or orifice, in the envelopes of the egg of the amphibia, at the time of, or before impregnation, capable of admitting the spermatozoon to the interior of the yolk-membrane or its contents.

"6th. That it is the spermatozoon alone which affects impregnation; and that this does not take place until the spermatozoon is brought into immediate contact with the external layer of the ovum.

"8th. That although direct contact of the spermatozoa with the ovum is indispensable to effect impregnation, I have never been able to detect any traces of these bodies in contact with the yolk-membrane, or even within the substance of the external envelope.*

"9th. That impregnation is *commenced* the instant the spermatozoa are brought into contact with the egg, but a certain *duration of contact* is essential to its completion.

"10th. That impregnation is not effected, when the whole or the majority of the spermatozoa in contact with the envelopes have previously become motionless, and apparently have lost vitality, as they are found to have done after the lapse of a longer or shorter period.

"11th. That although an exceedingly minute quantity of spermatozoa suffice to impregnate the ovum, the phenomenon of impregnation takes place more tardily even with duration of contact, when the number is extremely limited, than when it is in full abundance, without excess; while, when the quantity is deficient, or the duration of contact too limited, then the phenomenon is incomplete, and partial impregnation only is effected.

"12th. *Partial impregnation* is shown in imperfect segmentation of the yolk; and is due to the spermatozoa being insufficient in quantity, or in duration of contact, or inefficient through diminished vitality; and it may also result from diminished susceptibility in the ovum."

Newport's experiments and observations show, in brief, that contact alone of the spermatic particles with the ovum is requisite for fecundation, that each ovum requires several particles; and that there must be *duration* of this contact. Here is a limit to

* In his second series, Newport says he has detected the spermatic particles within the coverings of the egg, and sometimes even partially imbedded in the vitelline membrane beneath them;—but he had no evidence that they enter the vitelline cavity. We consider this fact of penetration within the envelopes as of no import in the question before us; for, as we hope to be able to show, it may well be regarded as contingent and not essential.

observation of physical facts, and we regard these important data worthy of full trust considering the source from which they come. This author discusses briefly the question of the nature of the impregnative power, and from the fact that the spermatic particles are sometimes seen to disappear on the surface of the egg-envelopes, he thinks it may be fair to conclude that the agency of this body is material in its operation; on the other hand, the fact of a mere *momentary contact* producing changes in the ovum, suggests in his mind the so-called catalytic power of certain known bodies. But he thinks that neither this last, nor endosmosis, are sufficient to account for the phenomena of this grand act.

The view of Keber, the last of the writers we have cited, has at least the merit of being unique if nothing more. As long ago as 1838, Martin Barry* announced that he had observed spermatic particles within the ovum. It should be mentioned however, that long previous to this, Prevost and Dumas† in their researches found these particles within the envelopes of the eggs of frogs. But Keber's alleged discovery is, that the introduction of the spermatic particles within the ovum, takes place through a special opening, a kind of micropyle, or an infundibuliform passage. This discovery was made upon the eggs of mussels (*Unio* and *Anodonta*), and that I may furnish a correct idea of its formation and relations, I will quote some of his conclusions. After describing the formation of the eggs and sperm of these animals, he proceeds:—

"3d. At the procreative season, there arises from the eggs, which are yet quite small, a bud-like process which projects from the albuminous membrane, and perforates the closely adjacent cortical membrane of the egg. This process bursts or dehisces, and there escapes from it a small quantity of albumen; it then receives a spermatic particle which has been introduced to the ovary in the usual way. This spermatic particle having thus been taken within the egg, the orifice of the micropyle closes up or becomes obliterated.

"4th. After this, and sometimes even before, the albuminous and vitelline membranes coalesce at the point of the micropyle; then the vitelline sac dehisces, and thereby the spermatic particle is received and enclosed in the very interior of the egg."—(p. 107.)

Upon this succeeds a description of the more or less minute changes which are alleged to supervene upon these processes, such as the disappearance of the micropyle, the disintegration of the spermatic particle, the singular changes of the vitellus which ensue, and the formation of a peculiar structure in the ovum for the ingress of the fecundating element—the spermatic particle, which finally reaches the very interior of the ovum and the germinative vesicle; but these need not detain us.

* *Barry*, Philos. Transact., 1840, pt. ii, p. 532.—1843, pt. i, p. 33.
† *Prevost* and *Dumas*, Ann. d. Sc. Nat., ii, p. 233.

The announcement of the presence of such a structure on the ovum is indeed wonderful, and more especally so since other observers, whose attention has been particularly directed to the embryological study of these animals, have failed to notice it, although one would suppose that an apparatus of this kind must be very visible. Keber affirms that he has observed a like structure in the ova of some other animals which he has examined. But, however well fortified he has sought to make his observations, they certainly need more than the usual confirmation, and we cannot but regard it as far from being a settled fact in embryology, that the ovum has a direct structural communication externally for the ingress of spermatic particles to its interior.

After all this discussion of facts, we revert to the primary question, what is the nature of the fecundating act? We have seen that its physical phenomena consist in the contact of active vital spermatic particles with the mature ovum; that this mature ovum, thus affected, experiences peculiar changes which terminate finally in the evolution of a new being possessing the characteristics of the male as well as of the female parent. It is true that, as was observed by Prevost and Dumas, and as has since been confirmed by Barry, Newport, and others, the spermatic particles may force their way through the envelopes of the egg some distance into its interior, but we regard this as an unessential condition of the fecundatory act; adhering by their heads to the envelopes of the egg, the incessant action of the tails of these bodies would obviously tend to force them inwards, and especially through such homogeneous, soft tissues as the egg-envelopes.

As a point of some importance in this connection, it may be mentioned, that there are cases where this intrusion of the spermatic particle into the interior of the ovum does not seem available from the very structure of the particle. Thus, with the Decapod Crustacea, these bodies consist each of a central nucleus to which are attached many long radiating processes; moreover, in these animals, the motion of the spermatic particles ceases so shortly after their escape from the body, that this entrance into the ovum would not be likely to be effected with even the most favorable perforating structure. It may also be here mentioned, that as motion is, with these particles, the only visible exponent of their vitality, it is highly probable that in these very same animals, the merest contact suffices to fecundate the ovum.

Even admitting the hypothesis of Keber and others, that the spermatic particle is mixed *de ipso* with the contents of the ovum, it would explain nothing, and would approximate us not in the least to the nature of this process, for it is just as comprehensible that the spermatic particle should fecundate by mere contact, as that it should by a material mixing or even an interpenetration of its consituents with those of the ovum.

By referring to the resultant phenomena of this fecundating process, we may perhaps gain some insight into the conditional if not the real nature of its agency. We have already said that the spermatic particle is the potential representative of the male; what signification is to be attached to its mere physical form, that is, whether it is conical, globular, &c., we know not; and this seems the more hidden from our perceptions, from the fact that exactly similar forms and sizes,—in fact, physical relations apparently identical—belong to spermatic particles of animals as widely dissimilar as could be. This fact alone, of the correctness of which we are well assured from our own observations, should be sufficient to convince us that we have here to deal with no very simple relations or properties.* But let us pursue the subject a little further. I scarcely need remark, that the offspring partakes equally of the physical peculiarities of both parents. It will at once be perceived, that in no way can the child receive those of the father except through the medium of the spermatic particles. And so, however strange it may appear at first, yet the conclusion is irresistible that spermatic particles must contain, concealed within them, in fact must be the vehicles of, not only the general peculiarities of the father as an animal, but mental dispositions also, and as is too often true of our own species, morbid taints superadded to all. It is wholly insufficient to say with Bischoff, that these anomalous conditions belong to the catalytic action; or with Newport that they may be the exemplifications of a force, peculiar and *sui generis*. For there is something above and beyond the wakening of latent forces, of one particle that is positive with another that is negative. The grand fact is, that the act of fecundation includes—whatever may be said of its also vitalizing the ovum—the communication or the transmission of the *individuality* of the male parent to the ovum; and the material organ of this transmission is, exclusively, the spermatic particle. We cannot see that these phenomena have an adequate explanation in any chemical relations of matter yet known, and what is more, we cannot conceive the possibility of this ever being the case, unless, indeed, chemistry gets beyond the domain of physics. Not but that chemistry involves points which are equally obscure, such as affinity, isomorphism, &c., but then the conditions of vitality, and especially the grand manifestation of it in question, certainly seem to us to include relations which have no correlation whatever with those pertaining to inorganic matter. To us the relations and conditions of cells, which are the primordial forms of organization, demand the *teleological* view of organic life.* *Individuality* is the distinguishing feature of organization, and we recognize in it something more than a mere collocation of physical conditions; we

* See The Relations of Cells to the Physical and Teleological views of Organization, in this Journal, xv, 87, Jan., 1853.

regard it as an Idea which exists before organization, which last as only the language in which the Idea is expressed. The conditions of this process of fecundation which we have just reviewed, will accept no other explanation, say what physiologists may about the *unphysical* character of such a view; we must have something beyond mere *combination*, which lies with physics; this we have in *development*, which lies with life.

In conclusion, we may say, that as the domain of science lies with demonstrable phenomena, so its legitimate study is with the sensible and tangible. The conditions of immaterial agencies, and their relations with material forms, must be accepted as pure phenomena incapable of the analysis of ordinary scientific facts. But after all, how much more of an enigma is the process of fecundation than the essence, the primordial cause of everything connected with both the inorganic and organic world about us. Science should put out her long, tentacular arms in all directions, laying hold of the tangible and the sensible, but it should be remembered that the supra-sensible is beyond her pale, and that "*multa esse constant in corpore quorum vim rationemque perspicere nemo nisi Qui fecit potest.*"

II. *Mikroscopische Anatomie oder Gewebelehre des Menschen;* von Dr. A. Kölliker, Professor der Anatomie und Physiologie in Wurtzburg. Zweiter Band: Specielle Gewebelehre.

Erste Hälfte; von der Haut, den Muskeln, Knochen und Nerven, mit 168 Holzschnitten, ausgefürht von J. G. Flegel, und vier lithographirten Tafeln.

Zweite Hälfte. 1. Abtheilung. Von den Verdauungs-und Respirations-Organen. Mit 127 Holzschnitten, ausgeführt von J. G. Flegel. 8vo, pp. 554, 346. *Leipzig*, 1850–52.

The writings of Kölliker already mark an episode in the history of physiological science. The department he has chosen, is *Structure*, in both its forming and its complete condition. This is a field which has been made worthy by the presence of most honorable names, who have marked it all over with intersecting lines which here and there stretch out beyond the ordinary boundaries, it is now so extended that it would, indeed, be meritorious to prove a faithful pilgrim in the paths of others in this wide domain. The merit and peculiarity of Kölliker is, that he has proved himself both a pilgrim and a pioneer, and while he has pushed exploringly upon hitherto untrodden ground, he has not, like many, been ignorant of what others before him have seen and done.

Of the work before us nothing has appeared like it, since the "Allgemeine Anatomie" of Henle, nearly twelve years ago, and the work of Henle was no more remarkable then, than that of Kölliker is now. Considering these two works, as we may, in a

measure, as corresponding books of their times, the difference in new matter or in newly developed points, as shown by an examination of the latter, is a satisfactory commentary on the progress of structural science during this brief period.

The second volume of the *Mikroskopische Anatomie* is published before the first, a mode of proceeding which we do not like, but which is becoming not uncommon abroad. It of course enables the investigator to put that portion of his labors before the world which have been finished, and so far is very good, but we question if the work, as complete, is not thereby delayed.

This second volume is devoted to special histology, and treats of the essential structure of the *skin;* or rather, more comprehensively, of the *cutaneous system*, of the *bones*, and the *nervous system;* these subjects are included in the first half. The first part of the second half, and all published that we have seen, treats of the digestive and respiratory organs; and when it is borne in mind that the histology of these two subjects occupies 900 pages, an idea will be entertained of the comprehensive manner in which they are taken up. The limits of our prescribed space make it wholly out of the question to review such a work, for each of its subjects presents enough of new matter originally brought out, for a special examination. We shall be well content if we succeed in noticing some of the more salient points in histology, here produced. It should, however, be first mentioned, that the mode in which Kölliker treats a subject is as comprehensive as it is different from that of any who have preceded him in works of this kind. In any given tissue, say, for instance, *muscle*, there are first described its peculiarities, general and intimate, as a tissue of its kind or genus; then follows the details of its structural analysis as species or varieties, from difference of location and perhaps special function; then its chemical and physical relations; then its comparative study with animals; then its development; then its pathology; and finally, its physiological relations and conditions. This relates of course to the simple histology of the tissue. But besides this, its vital relations are described, such as its innervation, its nutrition, and its connections with surrounding parts. You have then, Tissue: its structure, general, special, and comparative; its chemistry; its development; its pathology; and its physiology. The advantages of this mode of handling a subject of anatomy, aside from its comprehensiveness, must be apparent to all. I would here insist particularly upon the point of a comparative study of tissues in animals. Writers upon human anatomy and physiology have been too much inclined to treat the human subject with its details as though its physical relations to the inferior animals, were as dissimilar as those of its mind. They would seem to forget that, zoologically, man is an animal,

and therefore that his structures are but greater or less variations of the types of the classes beneath him. The truth is, human histology should never be studied until the student has passed through a course upon the lower animals; and all who have labored much upon the domain of structure know very well how the whole idea or plan of composition of a tissue has dawned upon them, by observing it in some of its simple and uncomplicated forms as occurring in an inferior animal. What is desired or aimed at, is the *formula* of a tissue, and this once being appreciated, can never be obscured or lost, whatever may be the language in which it is expressed. This statement obtains all the more force, from the unity and simplicity of plan pursued by nature, in the material forms through which her different organic conditions are manifested; thus, nervous tissue is the same, essentially, wherever found in the animal kingdom, whether in the plant-like polyp or in man. The same may be said of other tissues, and when the student of structure has fairly seized hold of such grand facts, he has proceeded not a little way on his course.

We cannot think of taking up any special portion of the work of Kölliker before us, not even in a general way, and quotations from so succinct a treatise would mar the sense and convey little meaning. Much, however, that is new and of special interest will be found under the head of *development* of each tissue or structure, for here Kölliker has shown himself particularly original and proficient. This is especially true of the nervous system, where there is a fullness of detail commensurate with the importance of the subject.

We look with more than ordinary interest for the appearance of the first volume of this encyclopædian work; but as it must comprise subjects which are yet hardly ripe for a satisfactory discussion, we would not ask its too early issue. We refer here to that debatable ground, *cells* and *cell doctrines*. Not satisfied, ourselves, of the universality of the application of Schwann's doctrines in this respect, and having, moreover, carefully traced the features of a new mode of cell-genesis essentially dissimilar from that of Schwann, there is some personal concern attached to this subject. But untied from any special theory, we shall be more than pleased if the distinguished Wurtzburg Professor is able to reconcile these dissimilar conditions of the science, and clearly show a single and universal mode of cell-genesis,—a unity, which, from the very correlative constitution of the human reason to the conditions of organization, the mind seeks and longs for in an earnest and almost prophetic manner.

Record of Anatomy and Physiology, Oct. 1, 1853.

SPECIAL WORKS.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By James Paget, F.R.S., &c. Vol. I. Hypertrophy: Atrophy: Repair: Inflammation: Mortification: Specific Diseases. Vol. II. Tumors. *London*, 1853.

De Spermatozoorum introitu in Ovula; Additamenta ad Physiologiam Generationis. Auctore Gotth. Aug. Ferd. Keber, Medicinæ et Chirurgiæ Doctore, Regio physico Insterburgensi. Accedunt lxxxi figuræ chalcotypicæ textui insertæ et in iv Tabulas collectæ. *Regiomonti Prussorum*, MDCCCLIII. (In German also.)

Beiträge zur mikroskopischen Anatomie und Physiologie des Ganglien-Nervensystems des Menschen und der Wirbelthiere. Von Carl Axmann, Doctor der Medicin. Mit 22 in den Text eingedruckten Abbildungen. *Berlin*, 1853.

Abhandlungen aus den menlichen und vergleichenden Anatomie. Von Wenzel Gruber, Dr. Med. et Chir., &c. &c. Mit xi Tafeln. *St. Petersburg*, 1852.

Mémoire sur la structure intime du Foie et sur la nature de l'alteration connue sous le nom de Foie Gras, &c. &c. Par A. Lereboullet. Accompagné de quatre planches. *Paris*, 1853.

Rudolph Wagner's Icones physiologicæ. Erläuterungstafeln zur Physiologie und Entwickelungsgeschichte. Vollständig neu bearbeitet und herau-gegeben von Alexander Ecker. Erste und zweite Lieferung. *Leipzig*, 1852.

[The first mentioned of these works will be found noticed in our reviews and the Bibliography of this number; and the last four deserve a special mention, for most of them are contributions to science of no ordinary character. That of Axmann (8vo, pp. 146), furnishes by far the most complete account we have of the microscopical structure of the ganglionic nervous system, and is well illustrated with numerous figures intercalated in the text.—The work by Gruber (4to, pp. 160), is composed of eight memoirs, the first seven of which relate to comparative osteology, and especially to some difficult points; the last memoir is upon human Myology, Angiology and Splanchnology, and the anatomist will be not a little surprised at the patient detail here shown. The whole is fully illustrated by 11 plates.—The work of Lereboullet (4to, pp. 117) is more important as bringing together in one work previous knowledge on the subject of the Liver, its structure and fatty degeneration, than as containing new facts. The essential ultimate structure, in fact the *formula*, of the liver is every where the same—that is cells which are the secreting organs. Fatty degeneration of the liver consists in these cells being loaded with fat globules. These two facts, long known in science, this author has brought out with much detail, as well also as the auxiliary hepatic structures.—What physiologist is not startled with pleasure at the announcement of *Icones physiologicæ?* A new edition of this renowned work is being published, and that too with appearances which would indicate that the hitherto enviable position of this work on physiological science, is not to be reduced even in these days of progress. Ecker, already well known in physiology, has undertaken this worthy yet difficult task. We have received the first and second Leiferungen as above announced; it contains twenty-seven plates, executed with surpassing beauty, which illustrate the Histology of the Splanchnic organs, of Muscle, of the Nervous System, of the Skin, of the Spermatic Particles, and of the Egg; then follows the Embryology of the Frog, and finally that of Man. It is now nearly thirteen years (in 1839) since the original *Icones* of Wagner made their appearance, and the illustrations then given were of such excellence, that they were copied so constantly as to lead to the omission of the acknowledgment of their source; and thus re-engraved, many of them as now seen in our various text-books resemble but little, at least in truthful beauty, their fine original. We can confidently predict the same fate for the illustrations of this new edition. In future notices it will be both our pleasure and profit to refer to this excellent work.]

PERIODICAL LITERATURE AND TRANSACTIONS OF LEARNED SOCIETIES.

Siebold & Kölliker's Zeitschrift für wissenschaftliche Zoologie. Bd. V. Hft. 1. August, 1853.

Leydig, Zur Anatomie von Coccus hesperidum. p. 1.

Gegenbaur, Ueber die Entwickelung von Doliolum, der Scheibenquallen und von Sagitta. p. 13.

Hannover, Zur Anatomie und Physiologie der Retina. p. 17.
Filippi, Ueber eigenthümliche Organe der Mundschleimhaut des Elephanten. p. 26.
Hessling, Ueber Seitendrüsen der Spitzmäuse. p. 29.
Leydig, Histologische Bemerkungen über den Polypterus bichir. p. 40.
—— Ueber die Vater-pacinischen Körperchen der Taube. p. 75.
Corti, Histologische Untersuchungen angestellt an einem Elephantem. p. 87.
Aubert, Beiträge zur Entwickelungs-geschichte der Fische. p. 94.
Gegenbaur, Ueber einige niedere Seethiere. p. 103.
Kölliker, Einige Bemerkungen über die Pacinischen Körperchen. p. 118.

Müller's Archiv für Anatomie, Physiologie und wissenschaftliche Medicin, 1853. Heft. 2, July.

O. Kohlrausch, Ueber das Schwellgewebe an den Muscheln der Nasenschleimhaut.
—— Ueber so genannte Infarkten.
E. H. Weber, Widerlegung der von Volkmann gegen meine Abhandlungen über die Anwendung der Wellenlehre auf die Lehre vom Kreislaufe des Blutes und insbesondere auf die Pulslehre gemachten Einwendungen.
J. Müller, Ueber den Bau der Echinodermen.

Henle & Pfeufer's Zeitschrift für rationelle Medicin. Neue Folge, III. Heft. 1, 2, 3. 1852-53.

Everts, Ueber Epilepsie. p. 1.
Weber, Ueber Croup und Tracheotomie. p. 8.
Donders, Die Bewegung der Lungen und des Herzens bei der Respiration. p. 39.
Von Dusch, Tetanus traumaticus; Chloroformnarkose; Genesung. p. 53.
Dursy, Beiträge zur Anatomie der Muskeln und Bänder der Hand. p. 65.
Böhmer, Ueber die syphilitische Affection der Leber. p. 88.
Zeucker, Sarcine in der Lunge. p. 117.
Nuhn, Versuche an einem Enthaupteten nebst erläuternden Versuchen an Thieren. p. 129.
H. Meyer, Ueber krebsige Phlebitis. p. 136.
—— Beiträge zur Lehre von den Knochenkrankheiten. p. 143.
Hilty, Der innere Callus, seine Enstehung und Bedeutung. p. 189.
Eckhard, Der galvanische Strom als Hinderniss der Muskelzuckung. p. 198.
Steinlin, Ueber Verrenkung des Unterkiefers. p. 204.
Krämer, Heilung eines sehr grossen s. g. angebornen Nabel- oder Nabelschnur-Bruches. p. 218.
Weinmann, Ueber die Absonderung des Bauchspeichels. p. 247.
Fuchs, Ueber Scabies crustosa seu norwegica Boeckii und deren Vorkommen in Deutschland. p. 261.
Fick, Ueber eine Methode mikroskopische Objecte mathematisch genau zu zeichnen und insbesondere deren Flächenräume zu messen. p. 273.
Kierulf, Einiger Versuche über die Harnsecretion. p. 279.
Donders, Beiträge zum Mechanismus der Respiration und Circulation im gesunden und kranken Zustande. p. 287.
Fischer, Beobachtungen über entzündliche Affectionen der den Bulbus umgebenden Gebilde in der Augenhöhle. p. 320.
Pauli, Ueber die Behandlung des Vorfalls der Gebärmutter. p. 328.
Meissner, Ueber Polypen des äusseren Gehörganges. p. 349.
Schleis, Glückliche Heilung einer Atresia recti ex retroversione vesicæ urinariæ congenita. p. 366.
Teichmann, Ueber die Krystallisation der organischen Bestandtheile des Bluts. p. 375.
Herbst, Die Unterbindung des Wirsung'schen Ganges an Kanischen, mit Rücksicht auf die Bernard'sche Ansicht über den Zweck des pankreatischen Saftes. p. 389.

Annales des Sciences Naturelles, xix, 1853. Nos. 2, 3.

Lacaze Duthiers, Recherches sur l'armure génitale femelle des Insectes hémiptères. (Suite). p. 65.
Koren & Danielssen, Recherches sur le developpement des Pectinibranches; seconde partie. (The first part is in tom. xviii, p. 257.) p. 89.

Pontaillié, Observations sur deux Distomes. p. 103.
Jules Haime, Observations sur les métamorphhoses et sur l'organisation de la Trichoda lynceus. p. 109.
R. Wagner, Note sur le développement des Vers intestinaux. p. 179.
Duvernoy, Mémoire sur les Oryctéropes du Nil blanc ou d'Abyssinie, et du Sénégal, suivi de nouvelles recherches sur la composition microscopique de leurs dents. p. 181.

Comptes Rendus, Tom. xxxvi, 1853, January to July.

Sinéty, Note sur une poche buccale chez le Casse-noix (*Nucifraga Caryoctætes*. p. 785.
Gratiolet, Rapport sur un Mémoire de M. Gratiolet sur l'organisation du système vasculaire de la sangsue médicinale et de l'Aulastome vorace, pour servir à l'histoire des movements du sang dans les Hirudinées. p. 841.
Dumeril, Mémoire sur l'organisation des Reptiles Batraciens que ont et conservent une queue pendant toute leur vie, ou Urodèles. p. 881.
Remak, Sur des fibres nerveuses ganglieuses chez l'homme et chez les animaux Vértebrés. p. 914.
Quatrefages, Recherches sur la vitalité des spermatozoides de quelques Poissons d'eau douce. p. 936.
Blanchard, Sur les movements du fluide nourricier chez les Arachnides pulmonaires. p. 1079.

Tom. xxxvii, to August 8.

Gratiolet, Recherches sur l'Anatomie de la Térébratule australe, pour servir a l'histoire des Brachiopodes. p. 45.

Proceedings Royal Society, London, January to June 23, 1853.

Toynbee, On the muscles which open the Eustachian tube. (Feb. 17.)
Lowe, On the Reproduction of the Toad and Frog without the intermediate stage of Tadpole. (Mar. 10.)
Barry, On animal and vegetable fibre as originally composed of twin spiral filaments, in which every other structure has its origin, &c.
— On the Penetration of the Spermatozoa into the interior of the Ovum; a Note showing this to have been recorded as an established fact in the Philos. Trans. for 1843. (Mar. 17.)
McDonald, Observations on the Anatomy of the Antennæ in a small species of Crustacean. (Apr. 7.)
Clark, On certain functions of the Spinal Cord. (Apr. 14.)
Neale, Researches on the distribution of the Blood-vessels, &c., in the lungs. (June 9.)
Allman, On the Anatomy and Physiology of Cordylophora, a contribution to our knowledge of the Tubularian Zoophytes. (June 16.)

The Annals and Magazine of Natural History, vol. xii, July, August, and September, 1853.

J. E. Gray, On the Operculum of the Genus Diplomatia. p. 9.
Quatrefages, On the Phosphorescence of some Marine Invertebrata. p. 15 and p. 180, (taken from Silliman's Journal, for March, 1853.)
Newport, On the Ocelli in the genus Anthophorabia. p. 44. (Proc. of Linn. Soc.)
J. Davy, On the eye of the Mole. p. 45. (Proceed. Zool. Soc.)
Lubbock, On two new species of Calanidæ, with observations on Spermatic tubes of Pontella, Diaptomus, &c. p. 115 and p. 159.
J. E. Gray, On the Teeth of the genus Mitra. p. 129.

ART. XLIV.—*Correspondence of M. J. Nicklès, dated Aug.* 20, 1853.

Reproduction of Cotton from Pyroxyline.—The following observation on the restoration of cotton from pyroxyline, was made by M. Béchamp, Professor at the School of Pharmacy at Strasburg. The process consists in heating pyroxyline, at the temperature of boiling water, with a concentrated solution of protochlorid of iron. The chlorid deepens in color and very soon there is a disengagement of pure nitric oxyd. When this disengagement has ceased, the process is ended, after washing the cotton with chlorohydric acid to remove the peroxyd of iron impregnating the cotton fibres.

By a similar method, M. Béchamp has succeeded in reproducing amidon from xyloidine, gum from nitric gum; and he has thus found a process which may be applied without doubt to many substances containing nitrogen in the state of NO_4.

Phenomena of Contact.—M. BERTHELOT, of whom we have spoken in our former contributions, and who has been engaged in some researches on forced dilatation, has undertaken the study of several points that have been much controverted. He has employed in his researches pressure aided by a high temperature. His apparatus and the observations he has made, may be useful in practical chemistry, and we propose to return to the subject in our next communication.

The following are some of his results:

1. Absolute alcohol heated in a closed vessel towards 360° C., along with *crystallized* chlorid of calcium, produces ether and olefiant gas. Cl Sr acts in the same way. Cl Ba and Cl Na he has under trial.

The chlorids hence act by contact, and not by removing the water of the alcohol, since they are used in the state of a crystallized hydrate.

2. Towards 400°, ClH NH_3 decompose alcohol, producing ether, a little olefiant gas, and chlorohydrate of ethylammine.

3. At 360° IH, AzN_3 acts in the same way: towards 400°, it decomposes ether into water and iodohydrate of ethylammine.

4. At 360°, wood-spirit, with crystallized Cl Ca gives methylic ether; with Cl H NH_3 at 300°, it affords methylic ether and chlorohydrate of methylammine.

5. Sugar heated to 100° C. with a little water is altered after some hours only in an insensible manner. With Cl Ca it changes to glucose. Cl Na does not act. ClH NH_3 produces rapidly this transformation.

6. Essence of turpentine in contact with acids at 100° C. is modified isomerically. Its rotatory power changes. This change takes place even when the acid is insoluble in this liquid, and enters into no combination. The essence of lemon with the acids at 100° C. undergoes a similar alteration.

The earthy chlorids and ClH NH_3, towards 240° C., act in an analogous manner on the essence of turpentine,

7. Fluorid of boron acts at the ordinary temperature. 1 part of gas (two vols.) transforms completely 160 parts of the turpentine without decomposition or evolution of gas. These changes are attended with a slight disengagement of heat.

Ammonia in Rain-water.—M Boussingault has been engaged for some time in investigating the proportions of ammonia in rain-water in the cities and country, taking Paris as one station, and for the other the old monastery of Liebfrauenberg, situated in the Department of the Lower Rhine on the eastern slope of the chain of the Vosges. The proportions of ammonia at Paris have varied from 1 to 5·45 milligrams, the mean, 3·35 milligrams per liter; while at Liebfrauenberg the rain-water contained hardly 1 milligram. Attributing the excess to emanations, Boussingault says, that Paris may be viewed as a vast mass of smoking chimneys—a comparison little flattering to a city which calls itself the "cerveau de l'univers."

Bronze for the Sheathing of Ships.—M. Bobierre, chemist at Nantes, who has studied this subject for some years, has arrived by his experiments, at the following conclusion: that by diminishing the proportion of tin the oxydizable metal is less uniform in its distribution through the plates, and there is a consequent inequality of alteration under the influence of seawater. His recent researches show that sheathing of bronze is preferable, as regards durability and solidity, to copper or brass. The abnormal alterations which have been observed are due to defective manufacture. The presence of arsenic does not occasion alteration in this alloy as happens for red copper. Bronze that will do good service contains in general 4·5 to 5·5 p. c. of tin; that with less, alters unequally. The introduction of a little zinc into these alloys of copper and tin, improves the product by favoring the diffusion of the positive constituent of the metallic mass.

Photography.—Several memoirs on photography have been addressed to the Academy. One of them relates to the cause of the partial reduction which the salts of silver experience in photographic operations, thereby damaging the results. M. Bertsch has recognized that these reductions are produced by hydrogen or its gaseous compounds, the presence of which cannot be easily avoided; a very small quantity suffices to produce a very evident effect on the bromids, chlorids and iodids of silver, even in perfect darkness. M. Bertsch mentions the remedy well known to chemists—the disengagement of some bubbles of chlorine gas in the place where the process is going on: this prevents these spontaneous reductions.

Other papers read before the Academy.—A note by M. Garnier completing his researches on the relation between the mean atomic weight of simple bodies and their specific heat, alluded to in the number for January.—A memoir of M. Carlet on Sebacic acid, discovered by M. Thenard, and obtained by M. Bouis by means of castor oil.—A note on the use of chlorine in analyses by MM. Rivot, Beudant and Daguin.—Memoirs of M. Payen and of M. Brame on marl beds and the effects of lime in agriculture.—Rules to be used in the administration of chloroform, by Dr. Baudens.—Notes of M. Favier, M. Bourdaloue, M. Breton de Champ, etc., on the levelling of the Isthmus of Suez; according to some, there is a considerable difference of level between the sea of Suez and that of Péluse, and according to others there is none at low tide. We wait for agreement between these inquirers. So also as to a discussion on the constitution of amids between MM. Gerhardt and Wurtz: we prefer to accept the facts and leave the method of interpretation of each to himself.

Galvanism.—A modification of Bunsen's battery is proposed by M. Guignet, consisting in replacing the nitric acid by a mixture of peroxyd of manganese and sulphuric acid. It is seen that the oxygen thus produced acts here the part of the nitric acid in burning the hydrogen proceeding from the decomposition of water. The process is recommended: but it is of too recent introduction to have been fairly tried.

M. Gautier communicates a fact of quite a different kind. He has obtained some sulphuric acid by exposing a mixture of air and sulphurous acid to the luminous arch produced by a powerful battery. To succeed, the points of the battery should be of platinum.

Dr. Amussat presents the results which he has obtained with the use of a galvanic battery in therapeutic surgery. With a platinum wire heated to a high temperature by means of a strong battery he has had complete success in the cauterization and removal of tumors. The wire should be incandescent, but must be used with caution, as at this temperature it is easily broken off.

Machine with Vapor of Ether.—The steamboat, Du Trembley, has an engine adapted for the use of the combined vapors of water and ether. The object of the ether is said to be to retain and utilise the heat lost by the steam, and employ it in forming a second vapor whose force shall be added to that of the steam.

The following is the process employed by Du Trembley, the inventor. The facts here given are from a report signed by an administrative commission which examined the invention during the first voyage made by the vessel between Marseilles and Algiers. The vessel is of a size to carry 100 passengers and 230 tons of merchandise.

In order not to lose the heat which the steam retains after employing its expansive force, M. Du Trembley receives the spent steam in a closed apparatus, consisting of a considerable number of small vertical cylinders placed close together, yet not in contact. The foot of these cylinders is plunged into a reservoir of ether placed under the apparatus which conveys the steam; the ether rises in the tubes, and in part fills them. When the steam has penetrated into the apparatus which traverses the tubes, and has surrounded them on all sides, the phenomenon mentioned above takes place; the water is condensed and the ether evaporates.

In condensing, the water produces a vacuum which adds to the expansive force of the steam in extinguishing the resistance which it had encountered; and the ether vapor, collected in a separate compartment in which the tubes terminate above the vaporizing apparatus, then contributes a new force which is added to that of the steam.

The condensed water is collected again in the boiler whence it had gone out as steam, carrying back as much heat as the ether had left after its vaporization.

The ether vapor which is collected above the vaporizer and the tubes in which it is formed, is taken into a cylinder specially adapted to it, but every way similar to that for the steam. The piston of the second cylinder can act either independently, or may connect with the same beam as that of the cylinder for the steam; in the latter case, the two vapors are combined in the same work, as takes place in the vessel Du Trembley, and which must always be the case in applying this system to navigation.

The ether vapor is treated like the steam: it passes into tubes like those of the vaporizer, where it is condensed by a continued jet of cold water which fills the apparatus and surrounds the tubes. The ether, again in the liquid state, is carried to the vaporizer, as the condensed water is restored to the boiler, to recommence the circuit just described.

Such is a brief description of the system of combined vapors adopted by Du Trembley. It may serve to explain the principle of his machine, and to show that compared with the ordinary steam engine, there will be a notable diminution in the fuel required.

In the course of the trials, the commission made four experiments on the quantity of coal expended. Together, these trials continued through 36 hours 50 minutes. They were made under different conditions as to weather; and whatever the winds, whether the ship was propelled by the engine alone, or by this with sails, the force of the engine was nearly constant at about 70 horses. The quantity of coal consumed during the $36^{h}\ 50^{m}$ was 2860·9 kil., or 77·67 k. per hour, and 1·11 k. per horse power.

Before using the *combined* vapors, the vessel was navigated with steam alone, under the same pressure, acting on the two cylinders. The amount of coal consumed during 28·18 hours, was 8519·5 k., or 302 k. per hour, and 4·31 to 4·51 per horse power.

The Report observes: "D'après ce calcul, on serait arrivé, par l'introduction de la vapeur de l'éther, à une économie de charbon sur la quantité dépensée quand les deux cylinders marchaient par la seule vapeur d'eau, de 3 kil. ·20 à 3 kil. ·38 par heure et par force de cheval ou de 74 kil. (26 p. 100), résultat si beau que nous osons à peine y croire, malgré que l'exactitude des chiffres sur lesquels nos calculs sont basés, nous soit de nouveau affirmée et qu'il diffère peu de celui que nous donnent nos propre experience."

As to the dangers attending the use of ether, they are neither more nor less than those inherent in the employment of illuminating gas, and as such reasons have not prevailed against this mode of illumination, they cannot prevail against an invention which has the advantage of actual and great economy.

As the ether is not lost during the work, the expense of it is small. It was the greatest difficulty encountered, so to close exactly the joints, as to allow of no escape of the subtle and inflammable vapor. But "this has been accomplished with such precision, that we can affirm," says the commission, "that if a slight odor of ether is perceived when the machine is heated up before starting, it disappears entirely after the ship is in motion."

Locomotion with the Vapor of Chloroform.—Chloroform has been used in place of ether in an engine somewhat similar to that of Du Trembley's. The experiments have been made in the Orient, on a steam vessel of 120 horse power, called the Galilee. They profited by the presence of the minister of marine, to run the vessel about the harbor in all directions. The details of the construction of the machine have not yet been made known. But after the above description of the ether engine, we may conceive that it is nearly the same thing. They speak of an economy of fuel of 50 p. c. with the chloroform engine.

Pisciculture.—The government of the Pays-Bas has instituted a commission for establishing in Holland some Piscines after Millet's system. A Piscine will be installed in the palace of the Voos in Guelder, another in the palace ("du bois") at the Hague. The commission will publish also a manual containing all the theoretical and practical data to insure good management and successful operations. The preparation of this manual is already far advanced.

Prizes Proposed.—The Société d'Encouragement, of which M. Dumas is president, proposes to issue the following prizes:—

1. A prize of 3000 fr. to the author of the best work on the nature of the disease which attacks the vine.

2. A prize of 3000 fr. to the inventor of the most effectual method of preventing or destroying the disease of the vine.

3. Three prizes of 1000 fr. each, and six of 500 fr. each, in favor of authors of the best works on the following subjects.

(1.) Origin and progress of the disease: the work to be accompanied by charts illustrating the annual progress.

(2.) Discovery of a method of sowing at will the Oidium or of inoculating it.

(3.) Discovery of the conditions of hybernation peculiar to the Oidium.

(4.) An exact history, accompanied with authentic proofs of the effects obtained from the use of different manures, and especially the sulphurous.

(5.) Variations in the disease due to climates, exposures, soils, and meteorological circumstances.

(6.) Historical account, accompanied with authentic documents, of the effects, positive or negative, obtained from the different remedies proposed and hitherto employed.

(7.) Research on the effects produced on the vine towards removing the disease by plants or trees in their vicinity, and especially those which are rich in volatile oils, or which inhale a strong odor.

(8.) Invention of apparatus convenient of use, for throwing different water solutions or powders on the vine.

(9.) Indications of the measures which may be prescribed by authority for preserving the green-houses, and even whole vineyards from the ravages of the disease.

The society at the same time issues a list of the different works which have been published on this subject, and will distribute all these documents gratuitously.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *New Bases containing Palladium.*—The chlorid of palladium, Pd Cl, forms with ammonia, as is well known, two compounds which have the same constitution but which differ strikingly in color, the one being dark rose-red, the other yellow. These compounds have been studied by Vauquelin, Kane, Fehling, and Fischer, and found to be represented by the formula $Pd\,Cl + NH_3$, but their true nature has remained unknown. Müller has given the subject a careful investigation

in Wöhler's laboratory and has arrived at the conclusion that the yellow salt is the chlorid of an ammonium in which one equivalent of hydrogen is replaced by one equivalent of palladium, and is consequently represented by the formula NH_3Pd,Cl. Müller terms the new radical NH_3Pd, *palladamin* which we shall however venture to change to *palladammonium*, corresponding to methyl ammonium, platin ammonium, &c. The oxyd of palladammonium is readily obtained by diffusing the chlorid through water and adding oxyd of silver, or by precipitating a solution of the sulphate by hydrate of baryta. In this manner a pale yellow strongly alkaline solution is obtained, which, when evaporated over sulphuric acid, yields a solid distinctly crystalline substance which is the oxyd NH_3Pd,O, and which may be redissolved without decomposition. The oxyd in solution absorbs carbonic acid rapidly from the air; it precipitates the salts of silver and copper, but dissolves no traces of the oxyds thrown down. When gently heated with a solution of sal-ammoniac, ammonia is given off, but on boiling, a new base is formed. With other ammoniacal salts the oxyd of palladammonium exhibits similar reactions, and it is therefore a stronger base than the oxyd of ammonium.

The salts of palladammonium are most easily prepared by decomposing the corresponding silver salts with the yellow chlorid. The author describes the carbonate, sulphate, and sulphite; they are all anhydrous; the two former are in yellow octahedra. The nitrate of palladammonium exists only in solution and is very unstable. The iodid of the new ammonium may be obtained in dark violet octahedra; the bromid strongly resembles the chlorid and gives yellow octahedra; like the chlorid these two compounds are anhydrous.

When the salts of palladammonium are treated with an excess of ammonia they pass into the salts of a second base corresponding in constitution to Reiset's platinum base and represented by the formula $2NH_3,Pd$. The author terms this base *palladdiamin*, but we shall call it *palladdiammonium*, as the termination *amin* has hitherto been applied only to ammonias and not to ammoniums. It may be considered as ammonium in which one equivalent of hydrogen is replaced by one of palladium, while a second equivalent of hydrogen is replaced by one of ammonium, the formula being $NH_2(NH_4)Pd$. The chlorid of this radical had already been obtained by Fehling in colorless prismatic crystals. Müller obtained the oxyd by precipitating the sulphate with hydrate of baryta; in this manner a pale yellow strongly alkaline solution was obtained, which by evaporation yielded a crystalline mass. The solution precipitates the salts of copper, iron, cobalt, nickel and alumina but not those of silver: it expels ammonia from a boiling solution of sal-ammoniac, absorbs carbonic acid from the atmosphere and forms salts when saturated in the cold with acids. An excess of acid, however, particularly when heat is applied, resolves the compounds of palladiammonium into ammonia and palladammonium. The author describes the chlorid, iodid, bromid and fluorid of this second radical, as well as the carbonate, sulphate and sulphite; all the salts are colorless and crystalline. Attempts to prepare a urea in which one-fourth of the hydrogen should be replaced by palladium, proved unsuccessful. By substituting ethylamin for ammonia, Müller obtained new bases, the chlorids of which were represented by the formulas $NH_2(C_4H_5)Pd.Cl$,

$NH_2(C_4H_8N)Pd.Cl$, and $NH(C_4H_5)(C_4H_8N)Pd.Cl$. An anilin compound corresponding to the first of these was also obtained; its chlorid is represented by $NH_2(C_{12}H_7)Pd.Cl$.—*Ann. der Chemie und Pharmacie*, lxxxvi, 341.

2. *Anthranilic and Benzoic Acids.*—GERLAND has instituted an accurate comparison of these acids, which together with a third acid discovered by Chancel, are represented by the formula $C_{14}H_6NO_3+HO$. The results of this investigation are as follows.

(1.) Anthranilic acid is the true carbanilidic acid constituted similarly to carbonic acid and represented by the rational formula CO_2, $CO(NH.C_{12}H_5)+HO$.

(2.) Anthranilic acid is different from benzamic acid and from the acid termed by Chancel carbanilidic acid.

(3.) Benzamic acid $C_{14}H_4(NH_2)O_3+HO$ is identical with Chancel's carbanilidic acid.

(4.) Nitrous acid NO_3 converts anthranilic acid completely into salycylic acid; the equation is $C_{14}H_6NO_3$, $HO+NO_3=C_{14}H_5O_5$, $HO+2N+HO$.—*Ann. der Chem. und Pharmacie*, lxxxvi, 143.

3. *New Compounds of Iridium.*—SKOBLIKOFF has discovered a class of ammoniacal compounds of iridium which exhibit a perfect analogy to the well known platinum bases of Gros and Reiset. The investigation was conducted in the laboratory of Woskressensky, and led to the following results. Protochlorid of iridium treated with carbonate of ammonia dissolves readily, and gives a greenish yellow, highly alkaline liquid, from which acids when added in very small excess, precipitate a yellow substance having the formula NH_3IrCl, and corresponding to the green salt of Magnus. Treated with hot nitric acid this substance yields a new salt which may be obtained in yellowish brilliant leafy crystals, represented by the formula $IrClN_2H_6O$, NO_5, and containing therefore a base exactly corresponding to Gros's platinum base, $PtClN_2H_6O$. The sulphate of this base crystallizes in fine greenish needles: the chlorid of the radical crystallizes in regular violet prisms, and is represented by $IrClN_2H_6$, Cl. By the action of dilute sulphuric acid upon the chlorid NH_3Ir, Cl yellowish orange leafy crystals are obtained which contain no chlorine, and are the sulphate of a base corresponding to platinammonium and palladammonium; their formula is NH_3IrO, SO_3 and the base may be called iridammonium. By boiling the chlorid of iridammonium with an excess of ammonia, a whitish substance is obtained which is the chlorid of a new base corresponding to Reiset's platinum base, and having the formula $N_2H_6.IrCl$ or $NH_2(NH_4)IrCl$. The author obtained a crystalline sulphate and nitrate of this base, having the formulas $NH_2(NH_4)IrO$, SO_3 and $NH_2(NH_4)IrO$, NO_5. The perfect parallelism between these bases, and those containing platinum and palladium is very interesting, and it is to be hoped that rhodium, ruthenium, and osmium will be studied from the same point of view.—*Journal für prakt. Chemie*, 58, 31.

4. *On Zirconia.*—In Katapleiite, a new mineral from Norway, SJÖGREN found an earth, the properties of which in general, corresponded with those of zirconia, but differed in the single particular, that the supposed new earth was easily soluble in an excess of oxalic acid. Berlin has investigated the subject carefully, and found that, contrary to the statements in all the text-books, the zirconia from the Frederichsvärn,

Expailly, Ural, East Indian, and Ceylon zircons is also easily soluble in oxalic acid, and that ammonia precipitates from the solution the hydrate of zirconia. The oxalate dried at 100°, has the formula $Zr_2O_3,2C_2O_3 + HO$, and is readily soluble in an excess of oxalate of ammonia. It is worthy of remark, that the precipitated hydrate of zirconia when long washed upon a filter absorbs so much carbonic acid from the air, that it effervesces when dissolved in acids. Berlin found the zirconia from Katapleiite, and from the Frederichsvärn zircon to have a density 4·9. He further states, that he did not succeed in separating zirconia into several earths, by the method of partial precipitations employed by Svanberg; the precipitates also contained the same amount of fixed base, and hence the equivalents of the different earths, if we admit the existence of more than one, must be the same.—*Journal für praktische Chemie*, 57, 145.

5. *On Didymium.*—Marignac has communicated a memoir on didymium, and its principal combinations. The author effects the separation of didymium from lanthanum by the method of Mosander, that is to say, by the crystallization of the sulphates. It was found however, that the process could be facilitated by first dissolving the oxyds in a large excess of nitric acid, and then successively adding small quantities of oxalic acid. The first precipitates are more deeply rose-colored, and more rich in didymium than the subsequent ones, and after converting the oxalates into sulphates, the separation of these last by crystallization is effected much more rapidly when one of the oxyds is present in much larger proportion than the other. The equivalent of didymium was determined at first by the analysis of its sulphate by means of chlorid of barium: it was found however, that the sulphate of baryta obstinately retained a portion of the sulphate of didymium from which it could not be freed by washing. The determination of the oxyd of didymium in a known weight of the sulphate, gave as a mean of five determinations 698·2 for the equivalent of the oxyd; the analysis of the chlorid gave the number 700·2 as a mean of these experiments, and the author adopts 600 as the equivalent of the metal. Metallic didymium was prepared, by heating the chlorid with potassium. A greyish powder was obtained, which in cold water disengaged bubbles of hydrogen; the particles of metal burned with vivid sparks when projected into the flame of a lamp. Two small fused grains of metal were also obtained, which had an iron grey color, and brilliant metallic fracture, tarnishing rapidly on exposure; they did not decompose cold water, but were rapidly dissolved by acids with a disengagement of hydrogen. The protoxyd of didymium is the only base which this metal forms with oxygen; it is easily prepared by igniting the nitrate, carbonate, or oxalate, and after strong calcination, is perfectly white, and is not peroxydized by moderate heating in the air, or by fusion with nitre. Addition of nitric acid however, and subsequent moderate ignition, produce a dark brown superoxyd, which again becomes white on stronger heating. The protoxyd is a strong base; it dissolves readily in dilute acids with a considerable elevation of temperature, and rapidly attracts carbonic acid from the air. It expels ammonia from its salts when boiled with them; but water slowly converts it into a hydrate. The hydrate as obtained by precipitation with potash, has a very pale rose color, and resembles alumina: the

salts of didymium have a pure rose or a faint violet color. The author describes a sulphid and an oxysulphid DiS and DiS+2DiO. The chlorid has the formula DiCl+4HO; it has a pale rose color, is deliquescent, very soluble in water and alcohol, and crystallizes in large oblique rhombic prisms; the solution cannot be evaporated to dryness without losing chlorhydric acid. The nitrate of didymium is anhydrous and deliquescent; the carbonate is a pale rose-colored powder, and is represented by the formula $DiO.CO_2+2HO$: the sulphite is also insoluble and has the formula DiO,SO_2+2HO. The sulphate has the unusual formula $3(DiO,SO_3)+8HO$; there is also a basic sulphate represented by the formula $3DiO,SO_3$. The double sulphates of didymium and ammonium, potash, and soda, are respectively represented by the formulas $NH_4O,SO_3+3(DiO,SO_3)+8HO$, $KO,SO_3+3(DiO,SO_3)$, and $NaO,SO_3+3(DiO,SO_3)$; they are pale rose-colored, very slightly soluble precipitates. The oxalate is completely insoluble in water, and almost insoluble in oxalic acid, and very dilute mineral acids; it contains four equivalents of water. The author thinks the didymium compounds analyzed by him, might possibly have contained traces of lanthanum, as the peroxydation of the protoxyd produces no sensible change in its weight.—*Ann. de Chemie et de Physique*, xxxviii, 148.

6. *Regeneration of Hippuric Acid.*—Dessaignes has succeeded in reproducing the hippuric acid from benzoic acid and glycosin, by causing the oxychlorid of benzoyl, to act upon glycosin-zinc oxyd. The reaction is represented by the equation

$$C_4H_5NO_4,ZnO+C_{14}H_5O_2Cl=C_{18}H_9NO_6+ZnCl+HO.$$

[This most beautiful result opens a new field of investigation, and promises to furnish us with a series of acids homologous with hippuric acid, since we have already two homologues of glycosin, alanin and leucin, and two of benzoic acid, namely, toluic and cuminic acids. The oxychlorids of the acetyl series should give a parallel series of new acids. —W. G.]—*Comptes Rendus*, xxxvii, 251.

7. *On the Alkaloids of the Quinquinas.*—Pasteur has studied the action of heat upon cinchonine and quinine, and has arrived at results of great interest. When sulphate of cinchonine is moistened with a little water and sulphuric acid and then heated, it fuses and remains fused even after the expulsion of all the water. If the fused salt be kept for three or four hours at a temperature of from 120° to 130° C., it is completely converted into the sulphate of a new base which the author calls *cinchonicine*, and which though entirely distinct from cinchonine, is isomeric with it. The sulphate of quinine treated in the same manner, yields another new base isomeric with quinine, and which the author terms *quinicine*. Pasteur has furthermore shown, that commercial quinidine is a mixture of two distinct bases, *quinidine* and *cinchonidine*, also isomeric with quinine and cinchonine, and like these yielding by heat quinicine and cinchonine. Quinine deviates the plane of polarization to the left, quinidine to the right, and both deviate it to a considerable extent. Quinicine deviates the plane of polarization to the right, but very feebly when compared with the other two. The same is true for the other three bases. The author suggests that the

molecule of quinine is double, and composed of two active molecules, one of which deviates strongly to the left, the other feebly to the right. When heat is applied, the sinistrorsal molecule becomes inactive, while the dextrorsal molecule resists the action of heat, and thus the new molecule of quinicine deviates feebly to the right. Quinidine on this view, consists also of two molecules, both of which are dentrorsal, but one much more powerfully than the other. Heat renders this component inactive, and the result is then the same as before quinicine is produced. Precisely the same explanation applies in the case of cinchonine and cinchonidine which both yield cinchonicine. Pasteur concludes, by stating that quinoidine is always the product of an alteration of the alkaloids, and that its formation may probably be entirely prevented by drying the bark after it is stripped from the tree in the dark. This would very greatly increase the product of quinine.—*Comptes Rendus*, xxxvii, 110, July 25th, 1853.

8. *Transformation of Tartaric acids into Racemic acid.*—PASTEUR has found that when tartrate of cinchonine is submitted to a gradually increasing temperature, it is at first converted into tartrate of cinchonicine; at a higher temperature the cinchonicine becomes quinoidine, while after an exposure of five or six hours at a temperature of 170°, the tartaric acid is in a great measure converted into racemic acid. The author considers that in this case the principal part played by the alkaloid, is to give stability to the tartaric acid, and enable it to support a high temperature without decomposition. Tartrate of ethyl which supports a high temperature without change, also yields a considerable quantity of racemic acid by heating. The racemic acid thus obtained artificially, is both physically and chemically identical with the natural acid, and may like this be split into dextro-tartaric and levo-tartaric acids. The levo-tartaric acid in combination with cinchonicine may also be transformed into racemic acid (common tartaric acid is dextrorsal.) After the separation of the racemic acid from the quinoidine, by precipitating it with chlorid of calcium, the filtrate from the racemate of lime deposits, after twenty-four hours, a salt of lime which contains a new species of tartaric acid easily obtained in fine crystals. This new species of tartaric acid exerts no action on polarized light, and cannot be split into levo-tartaric and dextro-tartaric acids. It differs therefore entirely from racemic acid. Pasteur has further found that it is possible to separate racemic acid into the two tartaric acids, by concentrating a solution of the racemate of cinchonicine to a certain point, when the levo-tartrate of the base crystallizes out. In the case of the racemate of quinicine the dextro-tartrate crystallizes first. The author throws out many ingenious suggestions, for which however, we must refer to the original paper.—*Comptes Rendus*, xxxvii, 162.

9. *Levo-camphoric and Dextro-camphoric acids.*—CHAUTARD and DESSAIGNES about a year since obtained a variety of camphor which deviated the plane of polarization to the left instead of the right, like ordinary camphor. On treating this camphor with nitric acid, Chautard obtained a new species of camphoric acid, which turns the plane of polarization precisely as much to the left as ordinary camphoric acid turns it to the right. The two acids—levo-camphoric and dextro-camphoric—resemble each other very strongly in chemical and physical properties. Equal

weights of the two acids brought into contact with each other immediately combine, forming a new acid which is altogether distinct from its two components, and which has no action on polarized light. The two camphors from which the two species of camphoric acid are derived, have also the same solubility, the same point of fusion and of volatilization, and the same power of rotation though in opposite directions. (It is not stated whether they unite to form a third and inactive camphor.) Pasteur has confirmed these results of Chautard.—*Comptes Rendus*, xxxvii, 166.

10. *On the Color of the Salts of Manganese.*—Gorgeu has at length settled the much disputed question of the cause of the pink color of the salts of the manganese, by proving that pure salts of protoxyd of manganese have always this color, and that the so-called colorless salts of this metal are in reality produced by the admixture of small quantities of the salts of other metals having the complementary green color. By boiling a solution of manganese with freshly precipitated and washed sulphid of manganese, iron, cobalt, nickel and copper are perfectly separated as sulphids, while an equivalent quantity of manganese is dissolved; the filtered solution gives crystals of a pink or rose-colored salt, yielding a pure flesh red precipitate with sulphid of ammonium. The author found that concentrated solutions of manganese are easily rendered colorless by the addition of small quantities of solutions of iron, nickel or copper: the decolorization is complete when the quantity of nickel is about $\frac{8}{1000}$ of the quantity of manganese; iron must be added in the proportion of $\frac{40}{1000}$, and copper in that of $\frac{1}{100}$, to produce a colorless solution, but in this last case, the solution in mass has a faint violet hue. The insoluble salts of manganese are white in powder, but rose-red when crystalline.—*Comptes Rendus*, xxxvi, 861. W. G.

11. *Note to Dr. Smith's paper on the Decomposition of Chlorids by nitric acid*, p. 373.—Dr. Smith's observation that the alkaline chlorids are decomposed by heating and evaporating with nitric acid is not new, although I am not aware that the fact has ever been published. Mr. H. Wurtz made experiments on the subject in my laboratory two years since, and obtained the same results as Dr. Smith. The complete decomposition of chlorid of magnesium by evaporation with nitric acid, was also observed in the laboratory of the late Prof. Norton, a year or two I believe before Mr. Wurtz studied the subject. The alkaline nitrates are easily converted into chlorids, by boiling them with an excess of chlorhydric acid, in presence of any metallic oxyd having a strong affinity of oxygen. I employ the protochlorid of tin for this purpose, and when the reaction is over, and gas (nitrous oxyd) is no longer evolved, a current of sulphydric acid gas removes the tin, and the filtrate contains only the alkaline chlorid and free chlorhydric acid.—W. G.

12. *On the Chemical Action of the Solar Radiations;* by Mr. R. Hunt, (Proc. Brit. Assoc., 1853, Athen., 1100.)—This was a report to the section of the continuation of an examination of the chemical action of the rays of the prismatic spectrum, after it had been subjected to the absorptive influences of different colored media. The mode of examination adopted has been to obtain well-defined spectra of a beam of light passing through a fine vertical slit in a steel plate by prisms of flint and crown glass and of quartz. The spectrum being concentrated

by a lens, was received upon a white tablet and submitted to careful admeasurement; the colored screen (sometimes colored glass and sometimes colored fluid) was then interposed, and the alterations in the chromatic image was carefully noted; the chemical preparation was then placed upon the tablet, and the chemical impression obtained. The relation which this image bore to the luminous image was a true representation of the connexion between the color of a ray, and its power to produce chemical change. In the report made to the Belfast Meeting of the British Association the results of experiments made upon glass tablets prepared by the so-called collodion process were alone given. In the present report the examination has been extended to the photographic preparation known as the calotype, and iodid and bromid of silver in their pure states and when excited by gallic acid. M. Edmond Becquerel, in a paper communicated to the Academy of Sciences, of which an abstract appears in the *Comptes Rendus*, tom. xvii, p. 833, states "that when any part of the luminous spectrum is absorbed or destroyed by any substance whatever, the part of the chemical rays of the same refrangibility is equally destroyed." The author's experiments, as recorded in the former report and those now detailed, prove that this conclusion has been formed too hastily. Although there are many absorptive media which, at the same time as they obliterate a particular colored ray, destroy the chemical action of that portion of the spectrum, yet there are a still more extensive series which prevent the passage of a ray of given refrangibility, and do not at the same time, obstruct those rays which are chemically active of the same degree of refrangibility. This is particularly exemplified in the case of glasses colored yellow by different preparations. With some of these the blue rays are obliterated, the chemical action of this part of the spectrum not being interrupted, whereas in some other examples those rays permeate the glass, but are almost entirely deprived of chemical power. A still more curious fact is noticed in this report, for the first time, of some media which have the power, as it were of developing chemical action in a particular part of the spectrum where the rays did not appear previously to possess this power. Several glasses exhibited this phenomenon to a certain extent, particularly such as were stained yellow by the oxyd of silver; but one glass showed this in a remarkable manner. This glass was yellow when viewed by transmitted light, but it reflected pale blue light from one of its surfaces; it obliterated the more refrangible rays down to the green, and rendered the yellow rays far less luminous than usual. In nearly every case the yellow rays are found to be not merely inactive, chemically, but to actively prevent chemical action. After the spectrum has been submitted to the action of this glass, *all chemical power is confined to this yellow ray.* The author has hitherto supported the view that photographic phenomena and the illuminating power of the sunbeam were distinct principles, united only in their modes of motion. He was led to this from observing that where there was the most light there was the least power of producing chemical change; and that as illuminating power diminished, the chemical phenomena of the solar rays increased. The results, however, which he has obtained during the brief sunshine of the present summer, leads him to hold that opinion in suspension. In many of the

spectra obtained (copies of which will be appended to the printed report) there appears to be evidence of the conversion of one form of force into another—the change, indeed, of *light* into *actinism* or chemical power; and, again, as in Mr. Stokes's experiments, the exhibition of the ordinarily invisible chemical rays in the form of *light*.

Prof. Stokes offered some remarks upon the different effects produced by the spectrum, dividing them into luminous effect, chemical action, calorific power, phosphorescence, and fluorescence. These were different effects resulting from the same cause, and he did not consider that sufficient evidence had yet been given to warrant the idea that there existed any dissimilar agencies in the solar rays.—Prof. Johnston, the Rev. V. Harcourt, Dr. Daubeny, Mr. Claudet, and others, took part in the conversation which followed.

12. *Magnetical Investigations;* by the Rev. Wm. Scoresby, D.D., F.R.S., L. and E., &c. Vol. ii, 448, pp. 8vo, with illustrations. London, 1852. Longman, Brown, Green and Longmans.—The special subject of this volume by Dr. Scoresby, is Terrestrial Induction, and in its elucidation which is elaborately carried out, the author has had in view as one object, its bearing on the navigation of iron ships. The world is indebted to Dr. Scoresby for great improvements in compass needles and other magnetic apparatus, and not less for his elaborate researches. The work treats, (1.) of General views of Magnetical Phenomena, and of the Earth's Inductive Action, illustrated with figures; (2.) of the action of terrestrial magnetism on iron and steel of different kinds, and on iron and steel of various proportions as to surface and mass; (3.) on the effects of percussion, etc., in aiding, modifying and neutralizing the inductive influence of the earth's magnetism; (4.) on the development of magnetical properties by percussion in steel and iron, as aided by contact with iron bars previously magnetized by hammering; (5.) indications explanatory of certain peculiar magnetic phenomena, derived from the foregoing results; (6.) on the application of principles affecting the magnetical condition of iron, to its real and thorough neutralization. Then the author enters upon the subject of the magnetism of iron-built ships, with relation to the action of the compass, and the changes to which their magnetic condition is liable, which is treated at length, with a consideration of all essential and many incidental circumstances.

13. H. W. Dove: Darstellung der Farbenlehre und optische Studien; 288 pp. 8vo, with 2 lithog. tables. *Berlin*, 1853.

II. Geology and Palæontology.

1. *Ueber die Vulkanischen Gesteine in Sicilien und Island, und ihre submarine Umbildung* von W. Sartorius von Waltershausen. 532 pp. 8vo. *Göttingen*, 1853.—This work by von Waltershausen is an elaborate discussion of the minerals and rocks of volcanic regions, especially those of Sicily and Iceland. The consideration of the chemical relations of the several species of the feldspar group occupy nearly 100 pages of the work (pp. 16–105). Other minerals are taken up in order and also the rocks, and many new analyses are given. The volume exhibits elaborate research throughout.

2. Statistique géologique, minéralogique, métallurgique et paléontologique du departement de la Meuse; par AMAND BUVIGNIER. 1 vol. 8vo, and atlas in folio. *Paris*, 1852.

3. Die geognostischen und orographischen Verhältnisse des nördlichen Persiens. Von Dr. C. GREWINGK. *St. Petersburg*, 1853. 8vo.

4. Untersuchungen über das Mainzer Tertiärbecken u. dessen Stellung im geologischen Systeme. Von Dr. FRIDOLIN SANDBERGER. *Wiesbaden*, 1853. 8vo.

5. Apunti sulla Geologia del Piemonte di BARTOLOMEO GASTALDI. *Torino*, 1853. 4to.

6. Zwei geologische Vorträge. Von OSW. HEER u. A. ESCHER von der LINTH. (1. Insects of the Lias. 2. Erratics of Switzerland.) *Zurich*, 1852. 4to.

7. Introduction philosophique à l'étude de la Géologie; par A. GAUTIER. *Paris*, 1853. 8vo.

8. Geognostiche Beschreibung des Siebengebirges am Rhein. Von Dr. H. v. DECHEN. *Bonn*, 1852. 8vo.

9. Description géologique et minéralogique du département du Bas-Rhin. Par M. A. DAUBRÉE. *Strasbourg*, 1852. 8vo.

10. Systematische Beschreibung und Abbildung der Versteinerungen des rheinischen Schichtensystems in Nassau. Von Dr. GUIDO und FRIDOLIN SANDBERGER. 5th No., folio. *Wiesbaden*, 1853.

11. Die Versteinerungen der Grauwackenformation in Sachsen, etc. Von HANNS BRUNO GEINITZ. 2d No. 4to. *Leipzig*, 1853.

12. Palæontographica: Beiträge zur Naturgeschichte der Vorwelt. Von Dr. DUNKER u. HERM. VON MEYER. 4to. *Cassel*, 1852. Vol. 1 is now complete. Of vol. 2, Nos. 1–6 have been published and No. 1 to 3 of vol. 3. (Compare Journal, September, '53, p. 280, No. 47.)

13. Traité de paléontologie ou Histoire Naturelle des animaux fossiles considérés dans leurs rapports zoologiques et géologiques; par F. T. PICTET. 2d edition, vol 1st. 8vo, atlas 4to. *Paris*, 1853.

14. Bericht über die Leistungen im Gebiete der Palæontologie mit besondererez Berücksichtigung der Geognosie während der Jahre, 1848 und 1849. Von Dr. C. G. GIEBEL. 8vo. *Berlin*, 1851.

15. Dimerocrinites oligoptilus. Ein Beitrag zur Kentniss der Gattung Dimerocrinites. Von R. PACHT. *St. Petersburg*, 1852. 8vo.

16. Description of the fossils of Syria; by T. A. CONRAD. 4to, with plates. From official report of the U. S. Expedition to explore the Dead Sea and the River Jordan; by Lieut. W. F. LYNCH, U. S. N. *Baltimore*, 1852.

17. Silicification organischer Körper. Eine geologische Abhandlung von ALEX. PETZHOLDT. *Halle*, 1853. 4to.

18. Fossile Flora des Uebergangsgebirges. Von Dr. H. R. GÖPPERT. *Bonn*, 1852. 4to, with 44 plates.

19. Iconographia plantarum fossilium. Von Dr. FRANZ UNGER. *Wien*, 1852. Fol., with 22 pl. (From the 4th vol. of the Trans. of the Imp. Acad. of Sc.)

20. Die Ammoniten von Swinitza. Von JOH. KUNDERNATSCH. *Wien*, 1852. Fol., with 4 pl. (From the Trans. of the Imp. Geol. Inst.) Vol. 1.

21. Die Gasteropoden der Gosaugebirge in den Nordöstlichen Alpen. Von Dr. L. F. SEKELI. *Wien*, 1852. Fol., with 24 pl. (From the same.)

22. Palæobromelia, ein neues fossiles Pflanzengeschlecht. Von Dr. C. von Ettingshausen. *Wien*, 1852. Fol., with 2 pl. (Id.)

23. Beitrag zur Flora der Wealdenperiode. Von Dr. C. v. Ettingshausen. *Wien*, 1852. Fol., with 5 pl. (Id.)

24. Die fossilen Mollusken des Tertiärbeckens von Wien, unter der Mitwirkung von P. Partsch, bearbeitet von Dr. M. Hörnes. Nos. 1 to 4. *Wien*, 1851–52. Fol., with 20 plates.

25. The Ancient Fauna of Nebraska, or description of remains of extinct Mammalia and Chelonia, from the Mauvaises terres of Nebraska. By J. Leidy. *Washington*, 1853. 4to, with 24 pl. (From the Smithsonian Contrib. to Knowledge.)

26. Description of an extinct species of American Lion, Felis atrox.

27. A memoir on the extinct Dicotylinæ of America; by Jos. Leidy. *Philadelphia*, 1852. 4to, with 4 plates. (From the Transact. of the Amer. Phil. Soc., vol. 10.)

28. Allgemeine Palæontologie. Entwurf einer systematischen Darstellung der Fauna u. Flora der Vorwelt. Von C. G. Giebel. *Leipzig*, 1852. 8vo.

29. Untersuchungen an Schädeln des gemeinen Landbären als kritische Beleuchtung der Streitfrage über die Arten fossiler Höhlenbären. Von Dr. A. Th. v. Middendorff. *St. Petersburg*, 1851. 8vo.

30. Ueber die Reptilien u. Säugthiere der verscheidenen Zeiten der Erde. Von Herm. v. Meyer. *Frankfurt*, 1852. 8vo. L. A.

31. Sur le gisement et sur l'exploration de l'or en Australia; by M. Delesse. 28 pp. 8vo. Extr. des Ann. des Mines, 1853, iii, 185.

32. Sur les variations des Roches Granitiques; by M. A. Delesse. 18 pp. 8vo. Extr. du Bull. Soc. Geol. de France, [2] ix, 464. 1852.

33. Recherches sur les Roches Globuleuses; par M. Delesse. 62 pp. 4to, with 4 plates. Extr. Mem. Soc. Geol. de France, [2], t. iv.—We propose to give an abstract of this memoir in our next number.

34. Über der orographische und geologische structur der Gruppe des Monte Rosa. Von Herm. Schlagintweit. 20 pp. 4to, with 2 plates. *Leipzig*, 1853.—Aus "Neue Untersuchungen über die physicalische Geographie und die Geologie der Alpen," von Adolph Schlagintweit und Herm. Schlagintweit.

35. *On the Production of Gold in the British Islands;* by J. Calvert, (Proc. Brit. Assoc., 1853, fr. Athen., 1104.)—From his own exploration, from researches in various works, and from communications, Mr. Calvert stated that gold was found in forty counties in the British islands, and over an area of 50,000 square miles. He thus classified the gold regions:—The West of England, North Welsh, Mid-England, of Northumbrian, Lowland, Highland, Ulster, and Leinster. The West England region might be divided into three districts—Cornwall, Dartmoor and Exmouth, or West Somerset. In Cornwall, the tin-streams which were of the same composition as gold-diggings, had long been known to contain nuggets and coarse dust, or hops of gold, but had only been slightly worked by Sir Christopher Hawkins, at Ladoch. The largest Cornish nugget was not worth more that about ten guineas. The Cornish districts were very rich in gold. The Dartmoor district contained gold in its northern and southern streams. A miner, named

Wellington, got about 40*l.* worth of gold, at Sheepston, and Mr. Calvert had obtained gold from the granite by this process. In the West Somerset were four companies for working gold ores. From 55 tons of Poltimore ores, 102 ounces of gold were lately reduced, being at the rate of 16 dwts. per ton, or twice the rate of the St. John del Rey ores. The West Somerset district probably embraced gold sites at Combe Martin and the Mendips. The North Wales district might embrace all the western counties of the principality. There were no reported river deposits, but gold ores had been worked at Carnhusian, Isso, Berthllwyd, Dolfrwynos, and other places. The Northumbian regions embraced Alstone Moor; but the chief gold-field was in Westmoreland and Cumberland. In the Goldscoop mine, gold had been found in the copper for ages, and he (Mr. Calvert) had discovered it in many of the ores and rivers of the district. He showed specimens from High Treby, Caldbeck Fells, the Buttermere and Crumwick-road, Borrowdale, Buttermere, Bassenthwaite, and a fine lump of gold gossan which weighed originally 57 oz. The South of Scotland district had only been worked for its river deposits in Clydesdale and Nithsdale, but in his (the lecturer's) opinion, it extended throughout the lowlands. Gold was found in about forty brooks or gullies, and all of the miners have gold for sale, obtained in their holiday excursions. Mr. Calvert mentioned that in the manuscripts of Queen Elizabeth's time, the diggers relied on keele, a reddish earth, as an indication of gold, and the miners do now. He had seen it also in Westmoreland, and had recognized it also in Australia and elsewhere. He found gold in the Lowther Burn, Long and Short Cleuch Burns, Mannock Water, Kepple Burd, Glengomar, Elvanwater, Goldscour, and other places. At Wanlockhead he saw gold in the midst of the town. At one place the miners, two years ago, got gold, which at Glasgow they sold for 42*l.* Gold was reported in Perthshire, Fifeshire, Stirlingshire, and Linlithgowshire. The Highland gold regions were unexamined. Gold localities had been reported in Aberdeenshire and Sutherlandshire. The Wicklow diggings were only shortly referred to. It appeared, by returns obtained from the Dublin goldsmiths, that the present supply of the peasantry was about 2,000*l.* a year. In Ulster the peasantry worked the aura or gold mountains in Antrimshire, and the Mayola streams in Londonderry yield gold. The yearly produce of gold in these islands was now about 5,000*l.* a year, which might be largely increased. The number of gold bearing streams known was one hundred. Gold had been found in nearly all the clay-slate districts. Many of these were worked in the Middle Ages, and probably also by the Romans. Gold, in ores, was found associated with silver, lead, copper, iron, and zinc; with quartz, granite, slate, oxyd of iron, sulphate of iron. These ores have been worked of late only in Devonshire and Merionethshire. The river deposits were rudely worked by the miners or peasantry in Wicklow, Lanarkshire, Antrimshire, and Devonshire. The washing of gold-stuff in our home districts was very rude, and not equal to that in Australia, nor had there been for a long time any deep workings. Many rich gold ores were thrown away, and much metal was produced from which the gold was not refined. The only two gold-fields which had yet been worked had yielded considerable amounts. The Lanarkshire district from a quarter of a million to half a million,

the Wicklow above 100,000*l*. The largest known nuggets were 3lb. from Lanarkshire, and others of 2½ lb. from there and Wicklow. The importance of attending to this branch of the national resources was strongly urged. Mr. Calvert concluded by stating that he considered the clay-slate formations of Canada would soon be discovered to be a vast gold-field.

36. *Reports of Professor Henry D. Rogers, on Wheatley Brookdale and Charleston Mines, Phenixville, Chester Co., Pennsylvania.* 40 pp. 8vo.—The lead-veins of Phenixville are very singularly productive in fine crystallizations of the ores of lead, and according to the report of Prof. Rogers, they appear to promise well in an economical point of view. The specimens of the sulphate, carbonate, molybdate, and phosphate of lead are of remarkable beauty.

The veins have a general parallelism, ranging with few exceptions, about N. 32°–35° E. by compass. They intersect a gneiss rock, and some of them also cut through an overlying red shale which Prof. Rogers states is probably of the triassic age, or perhaps of the older oolitic. They are therefore subsequent in origin to the shale. Numerous trap dykes also intersect the same red shale formation in this region as well as in the Connecticut valley. The lodes confined to the gneiss in general bear lead as their principal metal, and those which intersect also the shale contain ores of copper. Zinc ores (blende and calamine) prevail in both sets of veins, but somewhat more abundantly in the latter. The gneiss has a dip of 30 to 40 degrees, generally in the direction S. 20° E.; but there are many variations in the amount of dip near veins.

III. Botany.

1. *Harvey: Nereis Boreali-Americana; or Contributions to a History of the Marine Algæ of North America.* Part II. *Rhodospermeæ.* March, 1853. pp. 258, 4to. Plates 13–30; colored. Separate issue, from the fifth volume of the *Smithsonian Contributions to Knowledge.* —Our extended notice of the first part, published a year ago, sufficiently explains the nature and scope of this elaborate work. The present part contains a full systematic account of our North American *Rhodospermeæ*, the rose-colored Algæ, which are much more numerous than the *Melanospermeæ*, or olive-green Algæ, as well as more difficult to discriminate. Over a hundred pages more of letter-press and twice as many plates are therefore devoted to their elucidation;—a liberal allowance, under the circumstances; but twice as many figures might have been given to advantage, were it not for the heavy expense of such beautiful colored plates. The roseate hues and delicate texture of the *Rhodospermeæ* render them charming objects for such pictorial illustration; and Dr. Harvey's pencil is unrivalled in this department. But it is next to impossible to find colors so bright and clear as those of nature. The *Rhodospermeæ* are remarkable for having, in all their species, two kinds of fructification, or rather two kinds of spores, borne always on different individuals, and each equally capable of germinating and reproducing the species. The *idea* of each species, therefore, includes two individuals, not however as in certain

of the higher plants, and in most animals, where the pair is of two sexes, the sterile fertilizing the fertile one; but here there are two sorts of fertile individuals, which mature each their particular kind of seed, or spore, independently. These Algæ, moreover, like the Fuci, produce *antheridia*, which discharge myriads of granules, analogous to the pollen of Phenogamous plants, although they have not been observed to execute the spontaneous movements which they do in the *Melanospermeæ*. It has been commonly assumed that only one of these two kinds of reproductive bodies could be regarded as true spores, the other being supposed to be of the nature of a *gemmule* or bud, analogous to the bulblets of the Lilium bulbiferum, for instance, only of the utmost simplicity of structure. But authorities are by no means agreed as to which is the real spore, and which the gemmule. Our author (in his *Manual of the British Marine Algæ*) had very plausibly maintained that the smaller bodies, contained in a conceptacle, were the true spores, and that the *tetraspores* were gemmules. The same view has been proposed by Mr. Thwaites; while, if we mistake not, the contrary is supported by the younger Agardh. The question can only be settled by ascertaining which sort of fructification is fertilized by the antheridia. To us, it seems antecedently most probable, that these Algæ are truly dimorphous in fructification, and that both kinds are veritable spores, equally fertilized by the antheridia.

It may here be remarked, that some direct evidence of fertilization by the antheridia has lately been furnished, in the *Fucaceæ*, by M. Thuret, whose observations were communicated to the French Academy of Sciences last spring. The spores of certain common diœcious species, received in a vessel of sea water, apart from any admixture of the contents of the antheridia, uniformly failed to germinate. While those similarly circumstanced, to which the contents of the antheridia were added, promptly germinated. Impregnation is therefore inferred to take place in this family, in much the same manner as in the *Rhizocarpeæ*, (see vol. xv, of this Journal, p. 31, et seq.) that is, the matured spore receives the fertilizing influence after its separation from the parent.

The classification of the *Rhodospermeæ*, is more recondite than that of the *Melanospermeæ*; and most of the essential characters can only be made out by careful dissections, under the microscope. Dr. Harvey adopts, with some criticism on minor points, the recent elaboration of Prof. Agardh. We have, it appears, on the shores of North America, 385 species, disposed in 78 genera, under 13 "orders." The handsomest of all our species, namely that formerly known under the name of *Delesseria Americana*, here assumes the rank of a new genus, which Dr. Harvey has appropriately called *Grinnellia*, "in honor of Henry Grinnell, Esq., of New York, whose whole conduct in promoting the search after the missing Arctic Expedition of Sir John Franklin, justly entitles him to the respect and gratitude of every man of science."

Before the third and concluding part of this unrivalled work is put to press, the accomplished and indefatigable author may have the opportunity of personally gathering important materials for it, on the western coast of our continent; since he may return in that direction from the expedition he has undertaken to the Australian shores, via Ceylon and

Singapore, thus circumnavigating the globe in his search for the objects of his favorite study.

The volume of *Smithsonian Contributions to Knowledge* (vol. v), of which the above forms a part, is filled with natural, historical, and physiological memoirs. The following relate to botany. A. G.

2. *Plantæ Fremontianæ; or Descriptions of Plants collected by Col. J. C. Fremont, in California;* by JOHN TORREY, F.L.S.—The subjects, ten in number, are each illustrated by a plate. The first subject is *Spraguea*, (*S. umbellata*,) a remarkable new genus of Portulacaceæ, dedicated to our well known and unrivalled botanical artist, Isaac Sprague, who made the excellent figures which illustrate the present memoir, and whose profound knowledge of botanical structure, as well as the services he has already rendered to our science, justly merits this compliment. The second is the new genus *Fremontia* (*F. Californica*,) which replaces the genus formerly dedicated to Col. Fremont, (the earlier published *Sarcobatus* of Nees.) The present is a most interesting as well as showy shrub, being a Bombaceous plant, allied to *Cheirostemon*, the famous *Hand-flower* of Mexico, itself anomalous in the order, like the present genus, from the imbricated calyx and the want of a corolla. Its characters scarcely throw any additional light upon the affinities of *Cheirostemon*, which, as the older and best-known genus, should have given its name to the division of the order which Dr. Torrey proposes for these two genera. *Fremontia* is perhaps the most remarkable addition which has been made to the flora of the United States within recent times; unless the palm be yielded to the new Sarraceniaceous plant, presently to be mentioned. The third article illustrates *Librocedrus decurrens*, a new Californian Arbor-vitæ; a genus otherwise confined to South America and New Zealand, and perhaps to be reunited to *Thuya*. The fourth subject is the new genus *Coleogyne* (*C. ramosissima*,) of uncertain affinity, apparently most approaching *Chrysobalanaceæ or Rosaceæ*, (the author inclines to the latter,) notwithstanding the opposite leaves. The character which gives the name is a curious tubular sheath, belonging either to the disk or the andrœcium, which encloses the pistil. The fifth, *Emplectocladus fasciculatus*, is a new Rosaceous genus, probably of the tribe *Dryadeæ*; but the fruit is unknown. The sixth, *Chamæbatia foliolosa*, is a Dryadeous genus, indicated about the same time both by Dr. Torrey, and Mr. Bentham, and published by the latter in his *Plantæ Hartwegianæ*, but now admirably illustrated by the former. These articles include some valuable criticisms on the arrangement of *Dryadeæ*. Seventhly, in *Carpenteria Californica*, we have a new genus allied to *Philadelphus*, but 5–7-merous, and with an almost free calyx. Whatever evidence it furnishes in respect to the affinity of *Philadelphus* goes to show that that genus belongs to the order *Saxifragaceæ*, with which the "collateral relationship," as Dr. Lindley terms it, is so strong, that no difference whatever remains, except the convolute æstivation of the petals; which is of little moment while they are imbricated in *Fendlera*, and valvate with a slight modification in *Deutzia*. The eighth subject is *Hymenoclea*, Torr. and Gray, a curious genus of *Compositæ* allied to *Franseria*, of which last also two new species are described. The ninth is *Amphipappus Fremontii*, a Composita between *Gutierrezia* and *Solidago*, the brief characters of

which, as well as of *Hymenoclea*, had already been published. The tenth is *Sarcodes sanguinea*, a singular new genus of *Monotropeæ*, intermediate between *Hypopithys* and *Schweinitzia:* in this connexion are given some important remarks on this group of plants and the *Pyroleæ*, from which the author entirely excludes *Galax*. It should be remarked these genera date as far back as the year 1850, when the plates were engraved, and an abstract of their characters was read before the American Association for the advancement of Science, at the New Haven Meeting, and published in its proceedings.* A. G.

3. *On Darlingtonia Californica, a New Pitcher-plant from Northern California;* by JOHN TORREY, F.L.S. (With a plate.)—The foliage and scape of this plant, without flowers or fruit, was discovered by Mr. Wm. D. Brackenridge,† assistant botanist of the U.S. Exploring Expedition under Captain Wilkes, on the route from Oregon to San Francisco. The curious fish-tailed appendage to the hood of the pitchers, and the bracteate scape suggested the idea that the plant might be a new Sarraceniaceous genus, rather than a true Sarracenia. This proved to be the case, when flowering specimens were obtained from the same locality (near Shasta Peak) by Dr. G. W. Hulse; as these have several-flowered scapes, no calyculus, a very reduced proper lamina to the petals, almost definite stamens in a single row, a turbinate ovary with a depressed and dilated top, and, above all, a naked (five-cleft) style, without the umbrella so characteristic of *Sarracenia*. From *Heliamphora* (of Guiana,) it differs quite as widely; in fact it is a perfectly distinct third genus of this remarkable type, intermediate between the two before known. The only character it affords likely to throw additional light upon the affinities of the group, hitherto so obscure, is that of the almost definite stamens, which so far as it goes, favors Dr. Planchon's view, that it is related to *Pyrolaceæ*. We are well pleased that this most interesting and striking accession to the flora of our country, is to commemorate one of the oldest and best of our botanists, Dr. Darlington.‡ During the autumn and winter, living roots of this plant, packed in dry peat-moss, might be transported to the Atlantic coast, with good hope of success. Let our Californian readers take notice, that a small box of such roots, delivered alive in Boston, New York, or London, would be pecuniarily as valuable as a considerable lump of gold, and would furnish a handsome and highly curious acquisition to our gardens. A. G.

* We find that this and the two following memoirs are not included in the fifth volume of the *Smithsonian Contributions:* but a separate issue of them was published early last summer.

† Not J. D. Brackenridge, as the name is recorded, both in the Narrative of the U.S. Exploring Expedition, and in the present memoir.

‡ Our accomplished and venerable friend will doubtless be amused, and perhaps as much surprised as we were, to find that a German reviewer of the third edition of his *Flora Cestrica*, in the *Bonplandia* (a Botanical Journal published at Göttingen, and edited by B. Seemann), severely criticises his *English* style, pronouncing it to be so redundant in Americanisms, that he would advise Herr Darlington, should his work reach a fourth edition, to engage the assistance of some person better acquainted than himself with the idioms of the English language! We profess a somewhat critical acquaintance with our mother tongue, and must say that we had not noticed the Americanisms in question; nor had we ever expected to see Dr. Darlington taking lessons in the English language from a *Dutchman*.

4. *Observations on the Batis maritima of Linnæus;* by JOHN TORREY, F.L.S. (With a plate.)—This is a thorough history and illustration of this obscure plant, the plate filled with beautiful and clear analyses, from the pencil of Mr. Sprague; a fact not recorded upon the plate, as it should have been. A second species, *Batis Californica*, is indicated, which was discovered by Dr. C. C. Parry, near San Diego. As to the affinities of the genus, Dr. Torrey inclines to adopt the views of Dr. Lindley, (who refers it to his Euphorbial alliance,) but regards it as the type of a proper natural order. A. G.

5. *Plantæ Wrightianæ Texano-Neo-Mexicanæ;* PART II. *An account of a Collection of Plants made by Charles Wright, A.M., in Western Texas, New Mexico, and Sonora*, in 1851 and 1852; by ASA GRAY, M.D., pp. 119, with four plates. The greater part of this important collection was made under the auspices of Col. J. D. Graham, then in charge of the U. S. and Mexican Boundary Survey; the remainder, under his successor, Major Emory. The account of the collection is carried in the present memoir only to the end of Compositæ. A. G.

6. *A Flora and Fauna within Living Animals;* by JOSEPH LEIDY, M.D., (pp. 67, with 10 plates.)—Dr. Burnett has already called attention, in the last number of the Journal, to this elaborate memoir. From a botanical point of view, it may be pronounced to be a contribution of the highest order and interest; and the plates are unsurpassed, if not unequalled, by anything before published in this country. A. G.

7. *Exotic Fungi from the Schweinitzian Herbarium, principally from Surinam;* revised by the Rev. M. J. BERKELEY, M.A., F.L.S., and Rev. M. A. CURTIS, D.D. (From the Journal of Academy of Natural Sciences, N. Ser. vol. II.—A large number of species of fungi are here described, and three new genera are characterized, namely, *Coilomyces*, *Dasypora*, and *Corallomyces*. A plate is devoted to the illustration of these, and of certain species of several other genera. A similar memoir on the Schweinitzian Fungi of the United States is promised: it must prove a very important contribution to our mycology, which already owes so much to the individual and conjoined labors of Messrs. Berkeley and Curtis. A. G.

8. *Botanical Necrology.*—There have been several recent deaths among the botanists of Europe. The most important loss sustained is that of Professor ADRIEN DE JUSSIEU, the son and successor of A. L. de Jussieu (author of the *Genera Plantarum*), and the grand-nephew of Bernard de Jussieu, the correspondent of Linnæus, and the first to sketch the Natural System of Botany. M. de Jussieu, was himself one of the soundest and most learned botanists of the age, and a most estimable man, greatly beloved and admired by all who knew him. His decease will be greatly felt in Paris; both in the Academy of Sciences, of which he was this year the president, and at the Jardin des Plantes, with which he has been long connected. He died on the 29th of June, at the age of 56 years. He had long been in delicate health, and for the last four years had suffered grievously from an affection of the stomach, which proves to have been cancerous. Leaving no male heirs, with him closes the illustrious line of the Jussieus, which has been dominant in botany for more than a century. The botanical chair occupied by Jussieu at the Museum of the Garden of Plants, has now been

suppressed, and a chair of Paleontology established in its room, of which M. d'Orbigny is the incumbent.

About a year ago, another distinguished name in botany became extinct, by the death of M. ACHILLE RICHARD, Professor of Botany, &c., at the *École de Medécine*, and son of L. Claude Richard, one of the most distinguished contemporaries of A. L. de Jussieu. M. Richard was celebrated as a professor, and held no mean rank as a botanist. His chair in the Institute was filled by the election of Dr. C. Montagne, the well-known Cryptogamist; that in the Ecole de Medécine by M. Moquin-Tandon, formerly of Toulouse.

We hear of the decease of Dr. PRESL, of Prague, the author of the *Reliquiæ Hænkeanæ*, *Tentamen Pteridographiæ*, &c., &c. Also of the recent death of Dr. WALPERS, of Berlin, author of the *Repertorium*, and the *Annales Botanices Systematicæ;* a useful record of all plants recently published. He is said to have died by his own hands, after notifying his botanical friends that he was dissatisfied with the world, which would not recognize his merits. It is reported that Dr. Petermann, of Leipsic, will continue the *Annales*,—a work much needed,—or carry on a similar publication. A. G.

IV. ZOOLOGY.

1. Memorias sobre la Historia Natural de la Isla de Cuba; par FELIPE POEY. *Habana*, 1853. Vol. 1, No. 4.

2. Histoire des sciences naturelles au moyen age, ou Albert le Grand et son époque, considérés comme point de départ de l'école expérimentale; par F. A. POUCHET. *Paris*, 1853. 1 vol. 8vo.

3. A Naturalist's rambles on the Devonshire Coast; by PHILIP HENRY GOSSE. *London*, 1853. 1 vol. 8vo.

4. A synopsis of the Mollusca of Great Britain arranged according to their natural affinities and anatomical structure; by W. I. LEACH. *London*, 1852. 2 vols. 8vo.

5. Mélanges biologiques tirés du Bulletin physico-mathématique de l'Académie impériale des sciences de St. Petersbourg. Vol. 1, No. 6. 8vo. *St. Petersbourg*, 1853.

6. Der Harnstoff als Maass des Stoffwechsels. Von TH. L. W. BISCHOFF. *Giessen*, 1853. 8vo.

7. Uber die Larven u. die Metamorphose der Echinodermen. 4th paper. By JOH. MULLER. *Berlin*, 1852. 4to.

8. Ueber Synapta digitata ü. ueber die Erzeugung von Schnecken in Holothurien. Von JOH. MÜLLER. *Berlin*, 1852, folio with 10 plates.

9. Zur Naturgeschichte der Adria. Von L. K. SCHMARDA. *Wien*, 1852, fol. with 7 pl. (From the 4th vol. of the Trans. of the Imper. Acad. of Sci.)

10. Das Thierleben der Alpenwelt. Naturansichten u. Thierzeichnungen aus dem schweizerischen Gebierge. Von Fr. v. TSCHUDI. *Leipzig*, 1853. 8vo.

11. Indicis generum Malacozoorum supplementa et corrigenda. Auctore A. N. HERRMANSEN. *Cassellis*, 1852. 8vo.

12. Histoire naturelle des végétaux parasites qui croissent sur l'homme et sur les animaux vivants; par CH. ROBIN. *Paris*, 1853. 8vo, with atlas.

13. L'organisation du régne animal; par EMILE BLANCHARD. 5th No., folio. *Paris*, 1852. December.

14. Odontographie. Vergleichende Darstellung des Zahnsystems der lebenden u. fossilen Wirbelthiere. Von C. G. GIEBEL. 2d No. 4to. *Leipzig*, 1853.

We intend to give a detailed account of some of these works in the next number of the Journal. L. A.

III. *Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England;* by JAMES PAGET, F.R.S., &c. 2 vols. 8vo. London, 1853. Vol. I, pp. 499. (Hypertrophy: Atrophy: Repair: Inflammation: Mortification: Specific Diseases.) Vol. II, pp. 637. (Tumors.)

The Art of medicine is as old as humanity itself, but Pathology elevated to the dignity of a science which can be studied in an intelligent and definite way, is of quite recent origin.

Physiology and Pathology must always be mutual aids, for the latter is but a fallen condition of the former; and no one can clearly understand the physical conditions of disease, until he has learned the normal structural, and functional peculiarities of the parts in which it occurs.

The advance which Hunter gave to Pathology may be traced to this cause; his great store of physiological knowledge, derived too from direct study, enabled him, in the investigation of morbid conditions, to make that peculiar colligation of facts which has its expression in a *law*. The great hindrance of truth in the study of disease has been, that men have started upon the foregone conclusion that disease must be a foreign enemy, *sui generis*, which invades the healthy body; while they should have regarded it almost always, as merely a peculiar conditional form of the ordinary action of healthy parts, a leading feature of which is, that such action is *below* that of the normal standard. Perversion of the normal nutrition marks the more outstanding conditions of disease, and the leading tendency of modern research is to revive not a few of the ancient doctrines of humoral pathology, although based upon far more scientific data.

Mr. Paget's lectures were delivered some years since, and at that time were successively published in the London Medical Gazette. As they were then extensively read, their appearance in the present collected form, is not an event as important as though the work had not before been seen. It should be remarked, however, that the last of them were delivered last year (1852), and the whole have been put up in the present form with full reference and knowledge of advancing science. We wish particularly to call the attention of our readers to this work, not from its *practical* value in surgery, but because, in seeking to illustrate his subject, the author has taken up in a very philosophical manner, many of the fundamental physiological conditions of Life;—and in so doing has shown that he by no means ignores the relations of development and growth in the humbler forms of life, where many of these conditions have their clearest exemplification. Nutrition, its nature, purpose, and conditions, healthy nutrition, the formative process, growth, hypertrophy, atrophy, degeneration, reproduction of injured and lost parts, these are some of our author's preliminary studies, which are treated in a plain yet lucid manner, leaving no doubts in the mind of

the reader. These topics are taken up preparatory to a consideration of the more special process of the healing art involved in practical surgery. These it is not our province to discuss or even touch upon; but what we desire to impress is the necessity of studying the conditions of nutrition in all its phases of growth and repair, as illustrated in the lower forms of life where nature has laid bare the processes for our observation.

The study of the development, growth and repair of polypes, medusæ, and spiders, may seem to have the remotest connection with the domain of the pathologist, but the unity of plan by which nature proceeds in her processes, whether it be to develope a future or to recover a past perfection in the types of structure, makes the study of her humbler forms imperative with him who would have a wide comprehension of her laws. Were we so minded, this train of thought might be very pleasantly extended, and we could cite, beside our author, examples of men distinguished in their art, whose eminence is not a little due to that broad grasp of organic phenomena derived from scientific studies of this kind.

But we will quote one passage where Mr. Paget expresses himself on this very point, and with his usual felicity. It is near the conclusion of the seventh Lecture :—

"I may seem in this, as in some earlier lectures, to have been discussing doctrines that can hardly be applicable to our daily practice, and with illustrations drawn from objects in which surgeons may have but little interest. Let me, then, if only in apology, refer to some of the considerations which are suggested by studies such as these. Let me first, then, express my belief that, if we are ever to escape from the obscurities and uncertainties of our art, it must be through the study of those highest laws of our science, which are expressed in the simplest terms in the lives of the lowest orders of creation. It was in the search after the mysteries—that is, after the unknown highest laws—of generation, that the first glance was gained of the largest truth in physiology; the truth of the development of ova through partition and multiplication of the embryo-cells. So may the study of the repair of injuries sustained by the lowest polypes, lead us to the clearer knowledge of that law, in reliance of which alone, we dare not to practice our profession; the law, that lost perfection may be recovered by the operation of the powers by which it was once achieved. * * * * Then, let us not overlook those admirable provisions, which we may find in the lives of all that breathe, against injuries that, but for these provisions, would too often bring them to their end before their appointed time, or leave them mutilated to perish a painful and imperfect life. We are not likely to undervalue, or lose sight of the design of all such provisions for our own welfare. But we may better appreciate these, if we regard them as only of the same kind as those more abundantly supplied to creatures whom we are apt to think insignificant: indeed, so abundantly, that, if with a consciousness of the facility of self-repair, self-mutilation is commonly resorted to for the preservation of life."—Vol. I, p. 165, 166.

Pleasant as it would be to discuss these points and relations further, and to follow them in their close consequential detail to those methods of art which belong to the relief of humanity and the restoration of our

broken physical nature, yet this would hardly belong to our department. We shall be pleased if we succeed in calling particular attention to a work written in a fine philosophical spirit, with a true English practicality, and in a style excellent for its plainness and lucidity.

We may add that the "getting up" of these volumes is quite complete. Printed in a large clear type, and on excellent paper, it is indeed, a delight merely to read such writings. There are, moreover, constant wood-cuts intercalated in the text, which, for cleanness, are in keeping with the rest of the "work" of these volumes; and, finally, what we always like to see, there is a very complete and convenient index at the end. W. I. B.

V. Astronomy.

1. *New Comet*, IV of 1853, (Astr. Journal, 64.)—On the night of Sept. 11, 1853, Mr. C. Bruhns, in Berlin, detected near the forward paw of the *Great Bear*, a "large faint nebulous comet, resembling a star-cluster." Its position Sept. 11, $13^h\ 12^m\ 15^s{\cdot}3$ was R. A. $126°\ 59'\ 11''{\cdot}5$, and Decl. $+44°\ 51'\ 33''{\cdot}6$. Its position Sept. 15, $12^h\ 13^m\ 53^s{\cdot}8$ was $132°\ 22'\ 8''{\cdot}9$ in R. A., and $+42°\ 29'\ 54''{\cdot}5$ in Decl. Up to Sept. 17, no indications of a tail were visible, and the nucleus continued to present the appearance of an unresolved nebula, with numerous points of light. From the observations of Sept. 11, 13, 15, Mr. Bruhns had deduced the elements given below, which however, owing to the disadvantageous position of the comet, he considers as being only a tolerable approximation.

T. Oct. $16^d.6122$.

Long. of perihelion,	$301°\ 23'\ 20''{\cdot}4$
" asc. node,	$222\ 13\ 44\cdot1$
Inclination,	$63\ 6\ 5\cdot1$
Log. perihelion dist.,	9·200658

Motion retrograde.

2. *Proserpine*, (26) (Astr. Journ. No. 63.)—The following elements of this planet were computed by Mr. A. Krüger:—

Epoch, 1853, June 10·0.

Mean anomaly,	$358°\ 15'\ 12''{\cdot}1$	
Long. of perihelion,	$227\ 34\ 4\cdot6$	Mean Eqnx.
" asc. node,	$45\ 55\ 34\cdot3$	1853, Jan. 0·0
Inclination,	$3\ 36\ 14\cdot0$	
Angle of excentricity,	$4\ 20\ 30\cdot3$	
Log. semi-axis maj.,	0·418836	
" mean daily motion,	2·921752	

3. *Second Comet of* 1853, (Astron. Nach., 859.)—The elements given below, were computed by Mr. C. Bruhns, from observations of April 14, 16, and 19.

T. = 1853, May 10·39998 M. T. Berlin.

Long. of perihelion,	$201°\ 12'\ 57''{\cdot}2$	Mean Eqnx.
" asc. node,	$41\ 12\ 52\cdot3$	1853·0
Inclination,	$57\ 53\ 3\cdot0$	
Log. perihelion dist.,	9·956398	

Motion retrograde.

4. *Third Comet of* 1853, (Athenæum, Sept. 17.)—The comet which was so conspicuous near the western horizon about the first of September, reached its greatest brilliancy on the 3d of Sept.; on which day, shortly before one o'clock, P. M., the nucleus was perceived in full daylight by Mr. HARTNUP, of the Liverpool Observatory. It appeared round, and about 9″ in diameter, without any appearance of a tail.

5. *Shooting Stars of August* 9–10*th*, 1853.—In addition to the observations reported in the last number of this Journal, the following have been received. Mr. *Coulvier Gravier*, at Paris, reports (Comptes Rendus, tom. 37, p. 288) the hourly number on the 9th at 49, and on the 10th, 56, but omits to give the number of observers, and the time of night in which his observations were made.

Rev. Andrew T. Pratt, M.D., Missionary of the American Board, at Aintab, Turkey, has communicated some observations made by himself and another person Aug. 9–11th. The evenings were favorable, but their view was confined to the part of the heavens west of the meridian, and above the altitude of twenty degrees.

On the 9–10th, from 9 to 10 o'clock, they saw 36, and from 1 to 2 o'clock, 66. On the 10–11th, from 12 to 1 o'clock they saw 78, and from 1 to 2 o'clock, 88. The general direction of the meteors was quite uniform, the radiant being near *Cassiopeia.*

These observations agree with those heretofore made at New Haven as to numbers, general direction and greater frequency after midnight, and confirm, what may perhaps be considered as sufficiently well established, the cosmical origin of shooting stars.

VI. MISCELLANEOUS INTELLIGENCE.

1. *Note to Prof. Barnard's paper on Expenditure of Heat in the Hot-air Engine, page* 351; by the AUTHOR.—In Appleton's Mechanic's Magazine for September, it is editorially stated, on the authority of Capt. Ericsson, that the engines now in preparation for the caloric ship are to have working cylinders of 6 feet diameter and 8 feet stroke, are to be double-acting, and are to draw their supplies from a reservoir of highly condensed air kept artificially cold, to which the escaping air is to be returned. This construction requires no modification of the theory of the engine, but renders necessary, in the general formula, the substitution of the number expressing the pressure in the air-chamber, in place of 15, the pressure of the atmosphere. It is proposed to work against a pressure of six atmospheres—say 90 lbs. The mean positive working pressure will then be to that of the abandoned engines, as $90\times6^2 : 15\times14^2$, or as 54 : 49. The stroke, being also increased in the ratio 4 : 3, will increase the nominal horse-power, should the same number of revolutions be maintained, in the same ratio. The powers given in this table, therefore, being multiplied by $\frac{54\times4}{49\times3}=\frac{72}{49}$ will give the corresponding powers of the new engines.

The question of economy, however, remains unaffected, since the mass of air to be heated is increased in precisely the same ratio as the power. The mass varies as the product of volume into density;

that is, as $6^2\times8\times90 : 14^2\times6\times15$, or as $72 : 49$, the same ratio as the preceding. The success of the engine in this form will depend entirely upon the heating apparatus. Should that be no more efficient than the former, the failure will be more signal, because of the increased demand made upon it. It is to be presumed, however, that the inventor's attention is now directed mainly to this point, and that his attempts at improvement may not be entirely fruitless.

The formula for mean pressure (page 248 of this volume) may be made entirely general, by substituting a letter, as H, to stand for the pressure against which the engine works. It may also be rendered more simple in appearance by making the following substitutions. Put $k^{\frac{1}{\gamma}}=\mu$, and $\varkappa\frac{\gamma-1}{\gamma}$. With these changes, it takes the following form, which is here given in full, in consequence of the typographical error in the former article.

$$P=H\alpha\left[k^{\varkappa}m(\mu-l^{\varkappa}m^{\varkappa})+\frac{km}{1-\gamma}(l^{\gamma-1}-1)-\frac{l^{-\varkappa}m}{1-\gamma}(l^{\varkappa}-k^{\varkappa}m^{\varkappa})+m-1\right]$$

In Appleton's Mechanics' Magazine for July is contained a very able and satisfactory examination of the general theory of the caloric engine by Maj. J. G. Barnard, of the U. S. Engineers. As the article in this Journal for September appeared considerably later, in the order of time, it might naturally have been expected to contain some comparison of results with those of Maj. Barnard. This would have been the case but for the necessity of sending to the press before it was possible to see his article.* The difference in numerical results between Maj. Barnard and the present writer, are owing mainly to the difference of assumed maximum temperature in the working cylinder; this being in the one case put at 380° F. (the temperature observed on the trial trip of the "Ericsson,") and in the other, at 450° F. above melting ice, which has always been claimed by Capt. Ericsson to be a temperature attainable without difficulty. From what has already been said above, it will be seen that, at present, the writer is disposed to believe that Maj. Barnard has assumed the highest temperature attainable with the mode of heating heretofore employed. Whether the improvements which Capt. Ericsson has now in hand will remove this difficulty remains to be seen.

2. *The Conical Condenser, a Telescopic Appendage;* by Lieut. E. B. Hunt, Corps of Engineers, U. S. A., (Read before the Am. Assoc. for the Advancement of Science at Cleaveland, Aug., 1853.)—I would respectfully submit to the consideration of this Section, an arrangement which seems to make possible an almost indefinite increase in the space-penetrating power of telescopes. At the same time, I hardly anticipate that this arrangement will be of practical convenience; but whether it prove useful or not, it possesses a mathematical or optical beauty, which makes it worthy of presentation here.

* A similar reason must prevent, in the present number, any allusion to the continuation of Maj. Barnard's investigation, which, it is understood, is to appear in October.

It consists essentially of two conical mirrors of the same angle, and with a common axis, so placed that the beam of light is first incident on the inner surface of the exterior cone or ring-shaped section of a cone, and is thence reflected on to the exterior surface of the second cone, whence it is again reflected, in certain cases, parallel to its first direction. Thus the first or ring-shaped beam of rays is condensed, so that it can be received on the object-glass of a telescope, in condition for accurate convergence, the quantity of rays being greater as the ring-radius is increased.

To appreciate this, we first observe that if a ray be continually reflected between two parallel mirrors, its directions, after all the even reflections, are parallel to each other. Let the two cones now be assumed, with a common axis and equal vertex angles, the inner surface of the outer cone, and the outer surface of the inner cone, being reflectors.

First, take a beam of rays parallel to the common axis. Each ray of this beam which falls on the outer cone, and again, after reflection, on the inner cone, will emerge parallel to its primary direction, as the normals at its two points of reflection are parallel. Thus all the rays from the outer ring, however great its radius be, will in this case be condensed into a new beam parallel to the first, but emerging from the inner cone. An object-glass, or rather a telescope, with its axis coinciding with the cone axis, will thus receive a condensed beam, whose intensity exceeds that of the simple beam, in the same ratio that the orthographic projection of the effective ring by the incident-beam exceeds the area of telescope aperture. The condensing cones are thus optically a perfect arrangement for increasing the intensity of light from a star on the axis. Cutting off from the inner cone just enough to cover the object-glass, and from the outer one the part supplying rays to that portion thus determined, and mounting the two as a telescopic appendage, light may be condensed to any extent, from an axis-star, by increasing the ring-radius. The effect would be identical with that of enlarging the object-glass to an area equal to the orthographic ray projection of the ring. This ring could doubtless be increased to twenty or thirty feet diameter, which, with a two feet aperture would give such an illumination from an axis-star as is overpowering in contemplation.

Now, let a beam be taken, making, with the instrumental axes, a small angle, such as that of all beams in an astronomical field must always be. To trace this through the condenser, suppose the ring resolved into elementary rings, and the beam into corresponding elementary cylinder beams. Take the ray through the vertex of normals for an elementary ring, as an axis, and revolve all the rays of the elementary beam into the same plane. The rays fall parallel in revolution, and their points of reflection fall in an arc, subtending twice the angle between the axis of the beam and that of the cones. As the meridian planes in the elementary cylinder contain both the ray and the normal, all the reflected rays will cut the axis of revolution, and will be brought to accurate convergence on it, except for the spherical aberration due to the small arc above named. In the revolution we have effected, the points of second reflection will fall into a curve, dif-

fering not very much from a circle; by examining which, it will be seen, that it gives a counter or corrective aberration, which would make the final emergence nearly, in a beam, parallel to the first incident beam. Indeed, if we suppose an elementary beam to proceed, in inverse order, falling first on a ring element of the inner cone, it will have an emergent direction in all parts exactly parallel to that of the beam first reflected from the outer ring element. Thus the two reflections are strongly corrective of each other, and the incident elementary beam will emerge, after two reflections, nearly as a true beam, for all the extent of such a field as belongs to high telescopic powers. What is true of an elementary beam is equally true of the total beam.

It should be observed that the rays of each elementary beam lying in the plane through its axis and the axis of the cones emerge parallel to their first direction, and that for the adjacent portions of the ring, the lack of accurate emergence would be insensible. Thus, all points in the field have a perfectly correct defining part, in the condensing cones. Another important remark is that all incorrectly condensed rays are thrown *from* the centre of the field, thus leaving this part in its absolute perfection, and free from all possible confusion. The outer portion of the field is the least perfect, but even this has a correct definition, with less light, and some overlaying of wandering rays. To what extent around the axis the field would prove useful, experiment only can decide. As far as I can judge, this would be fully to the extent required in large telescopes, such as are now particularly desired for resolving the remote nebulæ, and in stellar observation in general. Much of the discussion of these mirrors, and their optical actions, I have omitted, as being superfluous here. My general conclusion is, that optically, this condenser is an available means of giving a vast increase to the space-penetrating power of telescopes.

The loss of light by each reflection should of course be taken into account, as a circumstance lessening the useful effect, in a degree, directly as the absorption, and depending on the material and polish of the reflectors.

Having presented the main optical properties of this arrangement, it remains to discuss its fitness for practical application.

Ordinarily, the most convenient plan would be to take cones whose elements make angles of 45° with the axis. This will bring the cone and ring, between the same limiting planes, perpendicular to the axis.

It would hardly be found practicable to append this condenser at the moving end of a large telescope with the present mounting. The size and weight would be found unwieldly, and would lead to frequent derangements of adjustment. If used at all, it must be, I imagine, by the introduction of a third plane reflector, so arranged opposite the inner cone as to deviate the emergent rays 90°, and to direct them through the telescope, horizontally fixed. This would enable us to mount a large ring on a firm circular frame, which revolves around one of its diameters, or rather around the fixed telescopic axis. The measuring arc of this revolution would give altitudes. This mounting would give only meridan observations. To observe at very limited distances from the meridian, a measureable rotation might be given to the plane mirror around its right line element, perpendicular to the telescopic axis. If

it be desired to observe through all azimuths and altitudes, the whole arrangement, including the telescope, can be made to revolve horizontally. Doubtless an equatorial mounting could be contrived, but I doubt if it could be made convenient.

The large ring would naturally be composed of segments, which could be cast in speculum metal, and polished by a revolving straight edge. These ring segments being developable, might be made of plate glass, cut into shape while plane, and bent, when heated, to the required curvature by a mould. Then a final polishing, and a pure silver backing, would complete them for mounting.

It is, perhaps, superfluous to dwell longer on this arrangement. I would say, in conclusion, that if an extension of telescopic power much beyond that now reached should be required, this arrangement presents a possible means of attaining it. At the same time, its complication, and its involving two or three reflections, will prevent its being so desirable as to overcome the difficulties incident to so novel a construction in this most difficult department of instrumental manufacture. As a means of condensing solar light or heat, I would ask particular attention to this apparatus. I see no reason why a heat of light focus may not be formed by the simple combination of these mirrors with a parabolic reflection, exceeding in intensity anything ever realized.

In using transit or other meridian instruments, it is now necessary for the whole telescope to turn in its axis, and for the observer to follow its rotation with much discomfort and torture of limb. There is a very simple mode of dispensing with all this, which occurred to Prof. Bache and myself, independently and nearly simultaneously. It has not yet been tried, and I know not when it will be.

Let the telescope be supported in any manner, with its axis horizontal and perpendicular to the meridian. Mount at its object-glass end a perfectly plane mirror, making an angle of 45° with the line of collimation, and arrange for this a rotation measured on a meridian limb. By this means, each point of the meridian will be passed in review, with no other motion than that of the mirror rotation. This mirror may be made a permanent appendage to the object-glass, or may be mounted quite by itself. The main difficulty would be in the first adjustment, and the setting up would then probably be easier than that of the existing transits.

A means of arranging this so as to get the internal reflection of glass, would be to compose the mirror surface of camera-lucida prisms, permanently fixed in échelon order. This, if perfectly executed, would be an excellent reflector, but it would be very costly. If silver mirrors can be formed which reflect 93 per cent. of the incident light at 45°, as Lord Rosse has found, they will be far the best solution of the problem. The electrotype art is surely so perfect in the Coast Survey Laboratory, that silver mirrors can be deposited on a speculum metal matrix, leaving only a final polish to be given. It is not rash to assume this, even though Lord Rosse failed in this very attempt.

Whether, on the whole, the introduction of this reflector, and the consequent revolutionizing of the transit, will prove desirable in practice, remains to be seen.

3. *Project of a Geographical Department of the Library of Congress.*—The following are the principal points in a plan for a geographical department of the Library of Congress, presented by Lieut. E. B. Hunt, U. S. Corps of Engineers, to the American Association for the Advancement of Science, at Cleveland, August, 1853.

I. That a Geographical department of the Congress Library be established, as a distinct and independently organized department, with its own executive officer,—the general direction being under the Joint Library Committee.

II. That special appropriations be made for this department, or that the Library Committee set apart a portion of the general Library appropriations for this purpose. During the period of collecting the great mass of existing materials, these appropriations would require to be proportionably large.

III. That the appropriated funds be applied to the collection, arrangement, and indexing of all important geographical materials relating to the whole world; also, in part to the necessary expenses of administration.

IV. Among the materials thus to be collected, the following classes may be mentioned: 1. A first-class terrestrial globe. 2. All materials illustrating the early and recent geography of the United States, both its sea-coast and interior, including traced copies of all valuable maps and charts in manuscript and not published. The materials for illustrating the past and present geography of each state, county, township and city, should be gathered by purchase, correspondence, and tracing. 3. All maps and charts on other parts of America. 4. The Admiralty or sea-coast chart of all the European and other foreign states, and the detailed topographical surveys of their interiors, where such have been made. 5. The most approved maps published from private resources, whether as atlases, nautical charts, or mural maps, including publications on physical geography, guide-books, railroad maps, and city hand-books. 6. A complete collection of all the narratives of voyages of discovery and exploration. 7. Geographical, geodetic and nautical manuals, and treatises, with all the requisite bibliographical aids to the amplest geographical investigation.

V. Having an organization and appropriations for gathering such a mass of material, it would be of the first importance to arrange complete and systematic indexes or catalogues which would at once make known all the material on each locality, and to have those materials so arranged as best to facilitate special research.

VI. A room for drawing, in which materials for the collection could be copied either for its files, or to answer public and private calls, would be indispensable for the completeness of this scheme. In this room, compilation could be conducted in answer to Congressional calls, and in keeping constantly corrected and filled out, a set of State maps on large scales, to which map publishers should have free access.

VII. A competent executive officer who would be able to maintain correspondence with persons having special geographical knowledge, and to keep a list of persons who could be addressed for additional information on foreign and domestic localities. Also, correspondence should be maintained with foreign geographical societies, and their publications should be secured with promptness.

VIII. The head of this department could present, through the Library Committee, an annual report, on the geographical explorations by our own and foreign Governments, or by individuals, so far as their results can be learned; making this, indeed, a synopsis of all the interesting and important geographical facts or publications for the year.

IX. Among the duties which would belong to this department would be that of calling attention to points demanding examination, or localities needing exploration. Also, it would be able to furnish the preliminary information for such explorations, or to indicate the sources whence it could be derived.

4. *On the Composition and Figuring of the Specula for Reflecting Telescopes*, (Proc. Brit. Assoc., 1853; Athen., 1098.)—Mr. Sollit commenced by stating that he had given his attention to this subject for years, and that he was more than ever convinced of its importance by the decided conclusion to which facts had led him, that reflectors, when once well and carefully made, were far less apt to deteriorate than refractors. In order to be intelligible to the section, it was necessary for him to go over some ground familiar to the public, since the researches of Lord Rosse, Mr. Lassell, and Mr. Nasmyth. He stated that he considered it to be a matter of prime importance that the copper and tin should be used in exact atomic proportions. He, following the numbers given by Berzelius, used the following proportions:—copper 32; tin 17·4. Lord Rosse's are, copper 32; tin 14·9. As the metal when thus composed was very hard, brittle, and difficult to work, he found that he could render it capable of reflecting white light equally well, if not better, and at the same time of taking a very uniform and beautiful polish, by introducing a little nickel in place of the tin,—and the following proportions he found on trial best:—copper 32; tin 15·5; nickel 2. He also found the introduction of a very small quantity of arsenic useful in preventing the oxydation of the tin when melting. Silver, as used by Mr. Lassell he also found excellent; but he was against the use of fluxes, as most injurious. The author passed over the casting and grinding with very slight notice; but dwelt on the composition and figuring of the polisher as of great importance. The composition as used by him was pitch and resin, and a small admixture of flour was found useful. The surface he grooved with concentric equidistant circular grooves,—and not in parallel and cross grooves, as used by Lord Rosse and Mr. Lassell. These concentric grooves he crossed by radial grooves, widening as they receded from the centre, so as to be bounded by curved outlines. By giving proper form and dimensions to these curves the parabolic form could be most accurately given to the spectrum in the process of polishing. The form of the curved outlines of these radial grooves he found should be parabolic. He concluded by stating the importance of not having the speculum too thin, and of using proper precautions in mounting and supporting it, to avoid any chance of the form being altered.

Dr. Scoresby regretted that having been in another section he had not heard the early part of the communication of Mr. Sollitt; but he rather thought Lord Rosse used concentric grooves in his polisher as well as parallel and cross grooves.—Prof. Stevelly confirmed the accuracy of this statement; and added that his memory was quite clear that Lord

Rosse considered it very important to use the copper and tin in atomic proportions, and said in his papers on it that uniformity of composition could not otherwise be hoped for. He also recognized the importance of using thick specula; the last which he had cast being not less than five inches thick. He also had used and recommended resin to be used to harden the pitch and flour for a purpose which by experience he had learnt to be important. Lord Rosse had also by the several motions and adjustments which he had contrived for the speculum and the polisher reduced the figuring of the speculum to an almost certain function of time: so that after the speculum had been a certain number of hours under the action of the polisher, he was well assured that the proper figure had been attained. Prof. Stevelly briefly described these motions and adjustments; and stated that the actual result was, an enormous circular disc of six-feet aperture, without crack or flaw, and of a splendid uniform polish, and reflecting light from objects of a perfectly natural tint.—Mr. VARLEY said he had found that the use of a little zinc in the composition of the speculum metal took from it the liability to tarnish which he had found so annoying. He expressed regret that Lord Rosse found it impossible to avoid microscopic pores in the construction of his speculum; his own experiments had led him to hope they might be avoided.—Mr. LASSELL said, if he had heard Mr. Sollitt correctly, he had said that he used silver in the composition of his speculum metal; now this was a mistake, as he used no silver in its composition. As to the proper proportion of tin to be used with the copper, he believed it to be impossible to give an unvarying rule, as the copper of commerce was very irregular in its quality and purity. He found the best mode to be to add nearly the quantity of tin known to be required,—which generally was from 14 to 15 parts tin, to 32 copper; and then weighing a small portion of that alloy, add to it by slow degrees known weights of tin,—and, assaying it from time to time by the simple test of dropping it into water as soon as it acquired a certain brittleness and brilliancy of fracture, easily to be recognized by practice, then adding in the same proportion to the whole alloy. He did not experience the difficulty from pores which had been alluded to, and he was not aware that Lord Rosse complained of it. His mode of casting most assuredly gave the portion of the speculum which was to be ground and polished free from them.—Mr. VARLEY said, up to a certain proportion of tin, the brilliancy and perfection of the reflecting power of the alloy seemed to improve, although its brittleness also increased; but beyond a certain limit the tin did not appear any longer to combine with the alloy,—for he has seen it in the process of cooling squeezed out, as it were, leaving the texture of the alloy spongy.

5. *On the Surface Temperature, and Great Currents of the North Atlantic and Northern Oceans;* by the Rev. Dr. SCORESBY, (Proc. Brit. Assoc., 1853; Athen., 1098.)—The author commenced by pointing out the great importance to physical geography of the subjects which he proposed to discuss, particularly as they tended, in the economy of nature, to furnish a compensating instrumentality against the extremes of condition to which the fervid action of the vertical sun in the tropical regions, and its inferior and more oblique action in the polar regions, were calculated to reduce the surface of the earth. Our knowledge of

all the currents of the ocean, with perhaps one exception, the Gulf-stream, which had been, in its more important features, carefully examined and surveyed, and more especially in the American Coast Survey,—was derived from the comparison by navigators of the actual position of the ship as determined from time to time with its position as calculated from what sailors technically called the "dead reckoning," or the course steered, and the distance run as determined by the log, an instrument by no means perfect. The determination, however, of oceanic currents, to which the present communication referred, depends simply on induction from observation of temperature, and that mainly of the surface. Such observations, indeed, only become available under considerable differences betwixt the mean atmospheric and oceanic temperatures: and where they may seem to indicate the region from which peculiar qualities of the sea are derived, they can afford but little, if any, information as to the precise direction or strength of the current so indicated, yet still the general results are found important and useful. The researches of the author embrace those in the Greenland Sea, the North Sea, and a considerable belt across the North Atlantic. To those in the North Atlantic he wished at present to direct attention; and to a belt of it embraced within the limits of a series of passages chiefly by sailing vessels between England, or some European port, and New York. Of these passages, sixteen in number, four were performed by the author himself, and twelve were supplied by an American navigator, Capt. J. C. Delano, an accurate scientific observer. The observations on Surface Temperature discussed, amount to 1153, gathered from a total number of about 1400. Usually Capt. Delano recorded six observations each day during the voyage, at intervals of four hours. Seven of the passages were made in the spring of the year,—two in the summer,—one in autumn,—and three in winter. Taking the middle day of each passage, the mean day at sea was found to be May 18th or 19th,—a day fortunately coincident in singular nearness with the probable time of the mean annual oceanic temperature. The author had laid down the tracks of the ship in each of the voyages, on a chart of Mercator's projection, and the principal observations on Surface Temperature were marked in their respective places. The observations were then tabulated for meridians of 2° in breadth, from Cape Clear, longitude 10° W., to the eastern point of Long Island, longitude 72° W., —embracing a belt of the average breadth of 220 miles, or a stretch of about 2,600 miles across the Atlantic. The results were the following: —1. Highest Surface Temperature northward of latitude 40°, 74°; lowest 32°; range 39°.—2. Mean Surface Temperature as derived from the means of each meridional section, 56°, whilst the mean atmospheric temperature for the corresponding period was 54°·2.—3. Range of Surface Temperature within each meridional section of 2°, 8½° at the lowest, being in longitude 20–22° W., and at the greatest 36°, being within the meridian of 62–64° W.—4. Up to longitude 40° the Surface Temperature never descended below 50°;—the average lowest of the sixteen meridional sections being 51°·88, and the average range 11°·3. —5. In the succeeding fifteen sections, where the lowest temperature was 32°, the average lowest was 37°·1 and the average range 29°·7. This remarkable difference in the temperature of the eastern and west-

ern halves of the Atlantic passage, the author said was conclusively indicative of great ocean currents yielding a mean depression of the lowest meridional temperature from 51°·88 to 37°·1, or 14°·8, and producing a mean range of the extreme of temperature on the western side of almost thrice the amount of the extremes on the eastern side,—or, more strictly, in the proportion of 29°·7 to 11°·3. The author drew attention to a diagram in which he had laid down along the entire belt curves showing the whole range of the lowest depressions of temperature and highest elevation, with the means at each longitude distinguished by different shading; and pointed out how the inspection of this as well as of the tabulated results affords striking indications of the two great currents, one descending from the Polar, the other ascending from the Tropical regions, with their characteristic changes of cold and heat. In classifying the results, the author considered the entire belt of the Atlantic track of the passages as divided into six divisions of 10° of longitude each, and these into meridional stripes of 2° each, omitting the first two degrees next the European end, or about 80 miles westward of Ireland to 72° W., or about the same distance west of New York. To each of these six divisions he directed attention, pointing out the conclusions to be derived from each. The curves approaching each other and running nearly parallel through the western half with great regularity, showing the variations and range to be much less, while throughout the eastern half the widening of the distance, and the irregular form of the extreme curves showed the influences of the two currents very remarkably. The author then proceeded to draw conclusions, showing that sometimes the cold current from the north plunged beneath the warmer current from the south. Sometimes they divided,—the colder keeping in shore along the American coast, the other keeping out and forming the main Gulf-stream Sometimes where they met they interlaced in alternating stripes of hot and cold water; sometimes their meeting caused a deflexion,—as, where one branch of the Gulf-stream was sent down to the south-east of Europe and north of Africa, and another branch sent up past the British Islands to Norway and Scandinavia by the Polar current setting down to the east of Newfoundland. The author next proceeded to consider the uses in the economy of nature of these great oceanic currents. The first that he noticed was the equalizing and ameliorating influence which they exercised on the temperature of many countries. Of this he gave several examples. Thus, our own country, though usually spoken of as a very variable climate, was subject to far less variations of range of temperature than many others in similar latitudes,—which was chiefly from the general influence of the northern branch of the Gulf-stream setting up past these islands. He had himself on one occasion in the month of November, known the temperature to rise no less than 52° in forty-eight hours, —having previously descended in a very few days through a still greater range; while in these countries the extensive range between mean summer and winter temperature scarcely in any instance exceeds 27°, and in many places does not amount to nearly as much. Another advantage derived from these currents was, a reciprocation of the waters of high and low latitudes,—thus tending to preserve a useful equalizing of the saltness of the waters, which otherwise by evaporation in low lati-

tudes would soon become too salt to perform their intended functions. Next he pointed out their use in forming sand-banks, which became highly beneficial as extensive fields for the maintenance of various species of the finny tribes, as in the great banks of Newfoundland. Next, this commingling of the waters of several regions tended to change and renew from time to time the soil of these banks,—which, like manuring and working our fields, was found to be necessary for preserving these extensive pastures for the fish. Lastly, by bringing down from polar regions the enormous masses of ice which, under the name of icebergs, were at times found to be setting down towards tropical regions, they tend at the same time to ameliorate the great heats of these regions, and to prevent the polar regions from becoming blocked up with accumulating mountains of ice, which, but for this provision, would soon be pushed down as extensive glaciers, rendering whole tracts of our temperate zones uninhabitable wilds. Dr. Scoresby concluded by pointing out several meteorological influences of these currents, by causing extensive fogs and winds more or less violent.

6. *On the employment of the higher Sulphids of Calcium as a means of preventing and destroying the Oïdium Tuckeri, or Grape Disease;* by Dr. Astley P. Price, (Proc. Brit. Assoc., 1853; Athen. 1100.)—Of the many substances which have been employed to arrest the devastating effects of this disease, none appear to have been so pre-eminently successful as sulphur, whether employed in the state of powder or flowers of sulphur, or by sublimation in houses so affected. Notwithstanding the several methods described for its application to the vines, I am not aware that any had been offered in 1851, when these experiments were instituted, by which sulphur might be uniformly distributed over the branches, and be there deposited in such a manner as to be to some extent firmly attached to the vine. Three houses at Margate, in the vicinity of the one in which the disease first made its appearance in England, having been for the space of five years infected with the disease, and notwithstanding the employment of sulphur as powdered and flowers of sulphur, no abatement in its ravages could be discovered,—I was induced to employ the pentasulphid of calcium, a solution of which having been found to act in no way injuriously to the young and delicate shoots of several plants, was applied to the juices in a dilute condition; the object in view being that the compound should be decomposed by carbonic acid, and that the excess of sulphur should be deposited with the carbonate of lime in a uniform and durable covering on the stems and branches of the vines. This was adopted, and although but few applications were made, the stems became coated with a deposit of sulphur, and the disease gradually but effectually diminished, in so much that the houses are now entirely free from any trace of disease or symptoms of infection. The young shoots are in no way injured by its application, and the older wood covered with this deposit of sulphur continues exceedingly healthy. This was, we believe, the first employment of the higher sulphids of calcium as a vehicle for the application of sulphur to the stems and foliage of diseased vines. Specimens were exhibited from vines which in 1851 were covered with disease, and which have since the autumn of that year received no further treatment. The vines in the imme-

diate neighborhood, and adjoining one of the houses, are covered with the disease, but, notwithstanding their close proximity, no indication of the disease has at present been detected in either of the three houses.

7. *On the Interior of Australia;* by A. PETERMANN, (Proc. Brit. Assoc., 1853; Athen., p. 1168.)—At a time when the exploration of the unknown interior of Australia was earnestly thought of, the probable character of that extensive region became a subject of particular interest, and of legitimate inquiry. Scarcely one-third part of Australia could be said to have been even partially explored, and by far the larger portion was therefore entirely unknown. This unknown interior of Australia had frequently been a matter of speculation, at first founded on very few facts. But, as our knowledge increased, and actual facts became more numerous, the theories had been modified. One of these hypothesis was, that the interior, to a certain extent, consisted of a shoal sea. It was in 1814, only forty years since, when the exploration of inner Australia might be said to have been systematically commenced, that Mr. Oxley, the first surveyor-general of New South Wales, a man of acknowledged ability and merit, surrounded about one-eighth part of the longitudinal extent of Australia. By tracing down the rivers Lachlan and Macquarie, he was checked in his progress westward by marshes of great extent, beyond which he could not see any land. He was, therefore, led to infer that the interior was occupied by a shoal sea, of which the marshes were the borders, and into which the rivers he had been tracing discharged themselves. The probability of this opinion was supported by the fact that the mouth of the largest of the Australian rivers, the Murray, had been overlooked by Capt. Flinders, and was not discovered till fifteen years after Mr. Oxley's discoveries, by Capt. Sturt. This opinion was adopted by subsequent travellers. In 1845, Mr. Eyre, one of the most distinguished explorers of Australia, in a paper communicated to the Royal Geographical Society, announced that he had arrived at different conclusions, namely, that the interior would be found generally to be of a very low level, consisting of sand, alternating with many basins of dried salt lakes, or such as were covered only by shallow salt water or mud, as was the case with Lake Torrens. He also said that it was more than probable there might be many detached and even high ranges, similar to the Gawler Range, and that, interspersed among these ranges, intervals of a better or even of a rich and fertile country, might be met with. In 1850, Mr. J. B. Jukes, in his valuable work on "The Physical Condition of Australia," stated his opinion to be that the interior consisted of immense desert plains, which seemed to extend to the sea coast round the Gulf of Carpentaria, or north to that of the Great Australian Bight on the south, and to stretch along the northwest coast to Collier Bay. The general opinion at present entertained on this point seemed to be very similar to that of Mr. Jukes, excepting, perhaps, that it was thought that the deserts did not reach so far to the north, and the northern parts were considered to consist of some fertile and promising regions. The chief grounds on which these deductions have been made were the known facts as to the climate and meteorology of Australia, and the absence of large rivers, besides other features. It was well known that the Australian colonies were subject in summer to occasional blasts of what is called the hot-wind, from its ex-

tremely high temperature. This hot wind always blew from the interior, in New South Wales and Tasmania, its direction being from the northwest and from the north in Port Phillip and South Australia. The breath of this wind was like the blast from a fiery furnace, increasing the mean temperature of a summer's day, on the westerly side of the eastern Cordillera, to 40°; on the eastern side, both in New South Wales and Tasmania, to 25° and 30°; and while during the hot wind the thermometer rose to 100° or even 115° in the shade, with the southerly squall there was sometimes a sudden fall of full 40° in the course of half or even quarter of an hour. This wind swept up from the interior clouds of dust and sand, sometimes intermixed with gritty matter large enough to strike with painful acuteness on the face. Count Strzelecki, while sailing from New Zealand to New South Wales, was prevented from making the harbor of Port Jackson for two successive days, by the violence of this hot wind. Though sixty miles from the shore, the heat exceeded 90°, and the sails of the ship were covered with a small powder by the breeze. The hot winds were, indeed, identical with the sirocco blowing from the great Sahara of Africa, and similar winds in other parts of the globe. It had been justly said that these hot winds, experienced in the southern parts of Australia, could have no other origin than by a current of air blowing over some large expanse of burning desert, and our knowledge of the adjoining regions entirely corroborated this assumption. The discoveries of Capt. Sturt, in his last expedition in particular, indicated the very nest and hot-bed of the winds. The situation of Capt. Sturt's desert was such that there was good reason to think its influences would extend to the whole of the coasts, even to those of Western Australia, which were the farthest from it, namely, about 1,350 geographical miles; unless the winds blowing from it were intercepted or deflected in the intervening spaces by mountains, or else ameliorated by countries of different character. The influence of the hot winds from the Sahara had been observed in vessels traversing the Atlantic at a distance of upwards of 1,100 geographical miles from the African shores by the coating of impalpable dust upon the sails. Mr. Petermann proceeded to describe the results of his investigations with respect to the causes of the hot winds, and observed that the heat of the winds in southern and eastern Australia was far more intense than in the north-eastern parts. He then remarked that he believed a great part of the interior of Australia to consist of sterile deserts; that the Torrens Basin and Sturt's Stony Desert formed the centre of the largest of these deserts, which probably extended from 200 to 300 miles around the latter, and that a fringe of 200 to 300 miles extended all along the great Australian bight to Western Australia, and along the western coasts as far as the Gascoyne Basin, or even the river Fitzroy. It also appeared to him that the whole of northwest Australia, north of Fitzroy River, as far as the head of Carpentaria Gulf, was a region of the most promising character, and that from this region a spur of more or less elevated land extended as far as the cluster of mountains discovered by Sir Thomas Mitchell, and which gave birth to many beautiful rivers flowing in all directions of the compass. This spur would necessarily form a bar between Sturt's desert and the Gulf of Carpentaria. It seemed to him most probable that this

promising district of northwest Australia extended far to the south, to the middle of the continent, and beyond it, at least to the latitude of Gascoyne River. One significant fact supported the latter opinion, and that was the occurrence of large trees which had been floated down the rivers of northwest Australia, and found at their mouths,—an occurrence unknown in southwestern Australia. In conclusion, Mr. Petermann said he could not but hope that a portion of this extensive and promising district would be explored and laid open for the benefit of mankind.

8. *On Preserving the Balance between Vegetable and Animal Organisms in Sea-water;* by R. Warrington, (Proc. Brit. Assoc., 1853; Athen., 1166.)—The public were first indebted to Mr. Warrington for a statement of the conditions in which animals could be kept in fresh-water without changing the water. It is not sufficient that there should be plants alone; but where the higher animals such as fish are kept, it is necessary that some beings should exist which will feed on the decaying vegetable matter. This desideratum is supplied by the various forms of phytophagous Mollusca. The author's success with fresh-water led him to try experiments with sea-water, and the results of his investigations were given in this paper. The most important fact established was, that marine animals could be kept in sea-water without changing in the same manner as in fresh. The conditions of the existence of sea-water creatures are, however, much more varied than those of fresh; hence the difficulty had been proportionally great in arriving at a successful issue. The nature of the plants in the first place is a matter of importance. The author found that the green sea-weeds answered better than the red or brown. In introducing animals they should be healthy and uninjured. Those should not be put together which devour each other. Crabs, especially the common crab, are very destructive; so are gobies, blennies, and rock-fish. The sea-water should be kept of a proper gravity. It should be 1·026 at a temperature of 60°. Rain or distilled water should be added from time to time to supply any loss. All dead animal or vegetable matter of any kind should be removed.

Dr. Daubeny stated that he had erected some fresh-water tanks at Oxford; but the difficulty which he had to contend with was the growth of Confervæ, which interrupted the growth of the other plants.—Dr. Walker-Arnott stated that he had no doubt the reason why the green sea-weeds answered better than the brown or red was that the latter were deep sea, whilst the others were shallow water, plants. The brown and red sea-weeds also had a much denser tissue externally than the green sea-weeds, and did not grow so fast.

9. *Appendix to the History of Vermont;* by Zadock Thompson. 64 pp. 12mo, with a map. Burlington, 1853.—This Appendix to Mr. Thompson's Vermont, is devoted to its Natural History, Geology, Meteorology and surface Features, in which departments it contains considerable original information. Among the discoveries of the author are the bones of the elephant, and of a new fossil whale. The latter is named by Mr. Thompson the Beluga vermontana, (this Jour., [2] ix, 256.) We cite the following altitudes of different places in Vermont, from page 6:—

ALTITUDES ABOVE THE OCEAN.

Mountain Summits.

		Feet.
Chin, *Mansfield*,	*Thompson*,	4348
Nose, "	"	4044
South Peak, "	"	3882
Camel's Hump, Duxbury, "		4083
Jay Peak,	*Adams*,	4018
Shrewsbury Peak,		4086
Killington, " Sherburne,	*Partridge*,	3924
Equinox, Manchester,	"	3706
Ascutney, Windsor,	"	3320
Snake Mt., Bridport,	*Adams*,	1310
Buck Mt., Waltham,	"	1035
Sugar Loaf, Charlotte,	*Thompson*,	1003
Snake Hill, Milton,	"	912
Cobble, "	"	827

Passes over the Green Mountains.

Lincoln,	*Adams*,	2415
Granville,	"	2340
Peru,	"	2115
Sherburne,	*Partridge*,	1882
Walden,	*De Witt Clinton*,	1615
Mt. Holly, (R. Road)	*Gilbert*,	1415
Roxbury, (R. Road)		997
Williamstown,	*Johnson*,	908

Villages.

Burlington Town House,	*Benedict*,	202
" University,	"	367
Milton Falls,	*Thompson*,	298
Essex,	"	452
Jericho Corners,	"	604
Underhill Flat,	"	665
Williston,	"	402
Franklin,	"	430
St. Albans,	"	370
Highgate Springs,		160
Swanton,		160
E. Berkshire,		460
Winooski Falls,		203
Sheldon,		375
Richmond,		332
Waterbury,		425
Middlesex,		520
Montpelier, (Capitol,)		540
Northfield, (Depot,)		724
Braintree,		732
West Randolph,		678
Bethel,		556
Royalton,		476
White River Junction,		335
Windsor,		288
Bellows Falls,		225
Woodstock,		400
Brattleborough,		160
Bennington,		432
Manchester,		650
Rutland,		500
Castleton,		475
Ludlow,		985
Proctorsville,		895
Chester,		670
Brandon,		460
Middlebury,		390
Vergennes,		225
Norwich,		400
Newbury,		420
Barnet,		460
St. Johnsbury,		585
Lyndon,		735
Barton,		953
Derby Centre,		975
" Line,		1050
Craft-bury Common,		1158
Troy, South,		740
Irasburgh,		875
Hardwick Hollow,		720
Hydepark,		560
Cambridge,		410
Johnson,		460

Lakes and Ponds.

Champlain Lake,*		90
Memphremagog "		695
Joe's Pond,	Cabot,	1544
Lyford's Pond,	Walden,	1692
Molley's Pond,	Cabot,	1626
Winooski Pond,	Peacham,	1410
Wells River Pond,	Groton,	1000
Crystal Pond,	Barton,	933
Mud Pond,	Sutton,	1183
Savanna Pond,	"	1210
Willoughby Lake,	Westmore,	1161
Elligo Pond,	Craftsbury,	893
Salem Pond,	Salem,	967
Pensioners Pond,	Charleston,	1140
Island Pond,	Brighton,	1182
Lake Connecticut, head of Con. River, in N. H.,		1589

Falls.

Great Falls, Marshfield, (head,)	1074
" " (foot,)	871
Nat. Bridge Falls, Waterbury (foot,)	345
McIndoe's Falls, Barnet, (head,)	449
" " " (foot,)	436
20 Miles Rapids, Lunenburg, (head,)	822
" " Barnet, (foot,)	486
Guildhall Fall, Guildhall, (head,)	835

* The level of Lake Champlain is taken for a basis in many of the surveys, which have been made, for canals and railroads, and their profiles indicate the height of places above the lake. In estimating, from these, the heights above the ocean, for the accompanying tables, 90 feet are added. The mean height of the lake above the ocean is frequently stated at 94 or 95 feet, but from the data, to which I have had access, I am disposed to think that 90 feet is nearly the true height. The change of level of the lake, that is, the difference between the extreme high water and the extreme low water marks, amounts to eight feet.

10. *On an Aurora Borealis of September 2nd, 1853*; by M. H. Boye.—I observed this aurora on the 2d of September last (1853) when on the Newfoundland Bank, about 10 miles south and 26 miles west of Cape Race, or in latitude 46° 25′ to 30′, and longitude 53° 25′ to 35′ (Greenwich). The previous evening an inconsiderable aurora had occurred soon after sunset, consisting of luminous clouds with few occasional streamers. On this occasion (the 2d of Sept.) the aurora commenced in the same manner not long after sunset, with luminous places and occasional streamers on the northern hemisphere, and also some on the east side of the southern hemisphere. Afterwards it assumed the appearance of three distinct arches of light, with their convex side upward; one in the N. W., being a kind of double arch, or having an undulation; a second of smaller size in east of north; and a third very large one beginning on the eastern side of the southern hemisphere, and gradually extending towards the western part of it embracing between its feet from one-third to nearly one-half of the whole horizon. The space below it was dark and without streamers, giving the appearance of limiting a cloud, but this could not be, as the stars were distinctly visible below it. From its upper convex edge, streamers would shoot up emerging towards a point below the zenith, in the southern hemisphere.

The space of radiant light, or corona, formed and disappeared many times, being most perfect at or about 9h. to 9h. 15 min. P. M. At the same time streamers would shoot up from all the arches, and in fact from every part of the heavens towards or quite to the radiant space. These appearances were repeated at very frequent intervals for more than two hours, from 8 to 10 o'clock. At times, and particularly towards the close of the phenomenon, the long streamers became more indistinct, and resolved themselves into innumerable dispersed lines of light terminating at their lower extremity, in a small bunch of short lines, giving the appearance of a golden rain or a golden figured drapery all over the heavens, which latter seemed to be illuminated on all sides. Altogether I consider it one of the finest auroras ever noticed, and particularly as regards the splendor and fullness of the light in the radiant space or corona, the sudden formation of which reminded me involuntarily of the sudden formation of crystals. The light of this aurora exhibited but a slight variation in colors, and there was some, but not much lateral motion of the streamers. Capt. Leitch of the steamer informed me, that he had been unable to observe any effect on the magnetic needle, the variation of which in this place he gave as 27° west, and which, as he paid particular attention to it, he would hardly have failed to do, had the effect amounted to 2° or 3°.

I have been informed, by several gentleman, that on the same evening an aurora was visible in Philadelphia, consisting of a band of light.

11. *On the Popular Theory of an Arctic Basin. Is it true?* by the Rev. Dr. Scoresby, (Proc. Brit. Assoc., 1853; Athen., 1167.)—The reverend gentleman proceeded to refute the notion of an Arctic basin, and observed that he was fully convinced of the futility of attempting to reach the North Pole by water. He believed he had penetrated farther into the Arctic Regions than any living man in the world—namely, to a

distance of 80½ degrees,—and his observations had left no doubt in his own mind that the country about the North Pole was one mass of stupendous blocks of ice. This opinion, he knew, was not by any means the popular one; but still he had not made his assertion without some claim to knowledge on the subject, and he would stake whatever reputation he might have as a traveller in those parts on the correctness of his statement. He firmly believed, however, that the North Pole might be reached by land, and in 1815 he had expressed that opinion in a work which he then published. In Edinburgh that opinion had been well received, but in England it was ridiculed as a wild and preposterous notion. Since then, Capt. Penny had undertaken an expedition on his plan, and though he had failed in the object of his mission, it was owing to circumstances over which he had no control. He (Dr. Scoresby) still thought the Pole might be reached. But by water it was, as he had said before, wholly impracticable.

A discussion ensued, in which Mr. Findlay and Dr. Shaw took part; the latter, after directing attention to the Arctic discoveries of Commander Inglefield, asserted his belief that it was by sea and not by land that a Polar basin is to be sought. The deeper the water the less ice is to be expected,—and a screw yacht might start from England in May, and return home in October with an account of a further penetration to the northeast between Spitzbergen and Nova Zembla than had as yet ever been accomplished by any navigator.

12. *On the Consolidation of Coral Formations.*—The paper by Prof. Horsford referred to on page 357, has recently appeared in a pamphlet form, and a copy has been received by the writer from the author. The paragraphs from Prof. Horsford's paper cited in my article in this number and the other statements referred to by me, remain in the pamphlet as they were published in the Traveller.—J. D. D.

13. *Daguerreotypes for the Stereoscope.*—The daguerreotype sent us by Prof. Barnard, as mentioned on page 350, is a very beautiful specimen of the art, besides having the peculiar interest which adapts it for the stereoscope. There are two admirable pictures, on the same plate, at the requisite distance apart for an ordinary stereoscope.

14. *British Association.*—The twenty-third meeting of the British Association was held at Hull during the week from the 7th of September last. The meeting for 1854 was appointed to be held at Liverpool.

15. *Clinochlore.*—The crystal of Clinochlore figured on page 436, vol. xv, of this Journal, was from the cabinet of William W. Jefferis of Westchester, Pennsylvania.

16. *Arago.*—The celebrated philosopher, Arago, died at Paris on the evening of Sunday the second of October. His age was 67 years, having been born February 26, 1786, at Estagel in the south of France.

17. *H. E. Strickland.*—We have to announce, with deep regret, the death of Mr. H. E. Strickland, who was killed on Wednesday by a railway train, whilst examining the strata of a railway cutting on the Manchester, Sheffield, and Lincolnshire line. The melancholy particulars are thus given in the daily papers.—"Mr. Strickland arrived at East Retford on Wednesday from Hull, having attended the recent meeting of the British Association. He was attached to the Geological Section of the Association; and in pursuance of his practical investigations in

that science, he proceeded on Wednesday afternoon to examine the strata of the deep cuttings on each side of the Clarbrough Tunnel, about four miles distant from Retford. A little after four o'clock, a boy at work in the fields observed him standing between the two lines of rails, near the mouth of the tunnel, on the Gainsborough side, with a pocket-book in his hand, apparently engaged in making notes. At this time, a coal train was approaching on the down line,—to avoid which, he stepped off the 'six feet' on to the up-line;—but unhappily he did so just at the moment when the Great Northern passenger train was issuing from the tunnel. The train dashed upon him,—and the next instant he lay a shattered and shapeless corpse."

Mr. Strickland was in the prime of life,—at that age when the promise of youth is fast realizing itself. He was born at Righton, in the East Riding of Yorkshire, on the 2nd of March, 1811. His father, Mr. Henry E. Strickland, of Apperley, in Gloucestershire, was a son of the late Sir George Strickland, Bart., of Boynton, in Yorkshire. He was a grandson on his mother's side of the celebrated Dr. Edmund Cartwright,—whose name is so indissolubly connected with the manufacturing greatness of England on account of his invention of the power-loom.

Mr. Strickland's boyhood was spent under his father's roof; where he was under the private tutelage successively of the three brothers Monkhouse,—one of whom is now a Fellow of Queen's College, Oxford. From his father's house he was transferred to the late Dr. Arnold,—who, prior to his appointment at Rugby, took private pupils at Laleham, near Staines. He finished his education at Oriel College, Oxford.

Although distinguished for his classical knowledge, Mr. Strickland had early acquired a taste for natural history pursuits; and after the completion of his studies at college he resided with his family at Cracourt House, near Evesham, Worcestershire—where he studied minutely the geology of the Cotswolds and the Great Valley of the Severn. Some of his earliest published papers were on geology; but his first effort as an author indicated a taste for the pursuits of his maternal grandfather. It appeared in the *Mechanics' Magazine* for 1825,—and was on the construction of a new wind-gauge.

In 1835, Mr. Strickland travelled in Asia Minor, in company with Mr. W. J. Hamilton, M.P.,—who was then secretary to the Geological Society. An account of this journey was published, in two volumes 8vo., by Mr. Hamilton, 1842, under the title "Researches in Asia Minor, Pontus and Armenia." This tour resulted also in the publication of several interesting papers on the geology of the districts visited, both by Mr. Strickland himself and conjointly with Mr. Hamilton. The principal papers published by Mr. Strickland singly were—"On the Geology of the Thracian Bosphorus."—"On the Geology of the neighborhood of Smyrna."—and "On the Geology of the Island of Zante." He early devoted his attention to the study of birds; and during this journey he gave proof of his ornithological knowledge by adding to the list of birds inhabiting Europe the *Salicaria Olivetorum*. He subsequently devoted a large share of his attention to the study of birds:—as his papers in the "Annals and Magazine of Natural History," and

Sir William Jardine's "Contributions to Ornithology" amply testify. His principal work, however, on this subject, and the one which will give him a place amongst the classical writers on the ornithology of this country, is devoted to the history of the Dodo. * *

Although as a zoologist, ornithology was his strong point, Mr. Strickland had an extensive knowledge of the various classes of organized beings. Thus, several of his papers were devoted to accounts of the Mollusca, both recent and fossil, in various districts. One of his papers at the last meeting of the British Association at Hull was, as our readers will see elsewhere, "On the Peculiarities of a Form of Sponge (*Halichondria saberea.*")

Mr. Strickland paid a large share of attention to the terminology of natural history,—and was the reporter of a Committee appointed by the British Association to consider of the rules by which the nomenclature of zoology might be established on a uniform and permanent basis. These rules were principally drawn up by him; and they have since their publication been very generally acted on,—and have contributed greatly to simplify Natural History nomenclature.

The general principles of classification could hardly fail to interest a mind so discursive as his,—and, accordingly, we find him at various times publishing on this subject. * *

It must be obvious, that the labors to which we have alluded imply an immense amount of industry,—but in the midst of all his practical investigations Mr. Strickland found time for purely literary work. Thus in 1847, he undertook to edit for the Ray Society a work, the collection of materials for which had cost Prof. Agassiz many years of labor, entitled "Bibliographia Zoologiæ et Geologiæ." Three volumes of this great work are published,—and the fourth and last is now in the hands of the printer. Mr. Strickland's labor here was not merely that of editing—it embraced the contribution of a large mass of additional matter, amounting to a third or a fourth of the whole. * *

On the occurrence of the illness of Dr. Buckland, and his withdrawal from the duties of the chair of geology at Oxford,—every one felt the propriety of inviting Mr. Strickland to deliver lectures in his place. Though young for so important a post, and with a reputation in other departments of science, he was found able to sustain the fame of his predecessor in this,—and brought to bear with great advantage the stores of his varied knowledge upon a science which is always susceptible of influence and amplification from the principles of other departments of science, however, distant from it they may at first sight appear.—The Reports of the British Association, the Transactions of the Geological Society, the papers of the Quarterly Journal of the Geological Society of London, and of the London and Edinburgh Philosophical Magazine, all testify to Mr. Strickland's activity as a geologist.

In several of the geological papers, Mr. Strickland's name is connected with that of Sir R. I. Murchison:—especially in a work on "The Geology of Cheltenham and its neighborhood." He assisted Sir Roderick in preparing for the press his great work on the Silurian system; and the proof-sheets of his new work on Siluria all passed through Mr. Strickland's hands,—the last of the work having been corrected at Hull.

In 1845, Mr. Strickland was married to the second daughter of Sir William Jardine, Bart. :—both of whom, with Mr. Strickland's father and mother, survive to lament his premature loss.

In the above brief sketch we have spoken only of Mr. Strickland's scientific career, but he had moral qualities that endeared him to all who knew him. Few came in contact with him who did not recognize in him a conscientious, amiable, and excellent man. In him Oxford has lost a Professor whom she could ill afford to part with at this time. To him, they who hoped for the wider culture of natural science at Oxford looked as to one who had the power and the ability to take the lead. The scientific societies have lost in him a member who was unwearied in his assiduity to carry out their objects in all their purity. His means made him independent of his labors ;—and all recognized in his exertions that love of science and its objects which constitutes the true philosopher.—*Athen.*, 1853, pp. 1094, 1125.

18. *Annual Report of the Superintendant of the Coast Survey;* showing the Progress of that work during the year ending November, 1851.—We allude again to this Report on the Coast Survey under Prof. Bache, to express our pleasure in the improved style in which it has been issued. It is a document of great value, in a scientific point of view, and deserves good paper, type, and binding, and these it now has. The valuable series of plates and sketches, instead of being folded into the octavo report make a 4to volume of themselves, which is convenient both for use and for their preservation.

19. *Twentieth Annual Report of the Royal Cornwall Polytechnic Society*, 1852. 108 pp., with tables. *Falmouth.*—Contains catalogues of plants and animals found at Falmouth, by W. P. Cocks, Esq.; observations on the Harvest in the east part of Cornwall, from 1816 to 1852, by J. Couch, Esq.: and an Essay on the China Stone and China Clays of Cornwall, with a description of some Mechanical Improvements in the mode of preparation of the latter, by Mr. H. M. Stoker.

20. *The Book of Nature;* an elementary introduction to the sciences of Physics, Astronomy, Chemistry, Mineralogy, Geology, Botany, Zoology and Physiology; by Friedrich Schoedler, Ph. D., Prof. Nat. Sci., at Worms, etc. 1st Amer. Edit. with a glossary and other additions and improvements from the second English edition, translated from the sixth German edition, by Henry Medlock, F.C.S., director of the Marlborough Street Laboratory and School of Practical Chemistry, etc. 692 pp. 12mo., illustrated by 697 engravings on wood. *Philadelphia*, 1853; Blanchard and Lea.—This volume, as its title shows, covers nearly all the sciences, and embodies a vast amount of information adapted for instruction. No other work that we have seen presents the reader with so wide a range of elementary knowledge, with so full illustrations, at so cheap a rate.

21. *The Ethnographical Library, conducted by* Edwin Morris, Esq. Vol. I. *The Native Races of the Indian Archipelago, Papuans;* by George Windsor Earl, M.R.A.S. 240 pp. 8vo. *London*, 1853, H. Bailliere.—Mr. Earl is well known as a writer on the Indian Archipelago and Australia. In this volume he treats of the Papuans of the East Indian Islands, describing their features, tribes, manners, social conditions, and implements, and various incidents in their history

since known to the whites, besides discussing to some extent the resources of the countries they inhabit. The work is illustrated by maps, and by sketches on stone of the natives, part of which are colored. The subject is to a considerable extent a novel one, and abounds in details of interest to the general reader as well as the ethnographer.

22. *Contributions to the Physical Geography of South Eastern Asia and Australia;* by G. W. EARL. 48 pp. 8vo., with a map. From the Journal of the Indian Archipelago, for May 1852. London, H. Bailliere.—The author briefly describes the general features and structure of some of the East India Islands, and their changes of level, but without many details by way of evidence, or a full display of precise facts in illustration of his subjects.

23. *Report of Progress of the Geological Survey of Illinois;* by J. G. NORWOOD, Geologist; communicated to the Illinois Legislature, at the session in 1853. 13 pp. 8vo., 1853.—This report announces the commencement of a Geological Survey of Illinois. The investigations promise to be well carried out under the direction and labors of Mr. Norwood. The state abounds in mineral products of economical value, as beds of iron, coal, mines of lead, building stones, etc. Its fossil remains are in vast numbers, and of high interest, and already rich collections have been made.

24. *American Handbook of the Daguerreotype*, giving the most approved and convenient methods for preparing the chemicals and the combinations used in the Art, and containing the Daguerreotype, Electrotype, and various other processes employed in taking Heliographic Impressions; by S. D. HUMPHREY. 110 pp. 12mo. *New York*, 1853.—Much practical information is embraced in this little work and it will be valued by those interested in the art of which it treats. Some of the chemistry however requires a revision.

JOURNAL OF THE UNITED STATES AGRICULTURAL SOCIETY; Published Quarterly. No. 1. July, 1853. 160 pp., 8vo. *Philadelphia*, 1853.

THE PEOPLE'S MEDICAL GAZETTE; edited by John Davis, M.D., Practising Physician. Published monthly, $1,00 per annum. Vol. I, No. 1, September, 1853, pp. 32.

THE AMERICAN COTTON PLANTER; A Monthly Journal devoted to Improved Plantation Economy, Manufactures, and the Mechanic Arts; edited by N. B. Cloud, M.D., La Place, Alabama: begun with January, 1853, and issued monthly at $1 per year.

THE MEDICAL REPORTER; A Quarterly Journal published under the direction of the Chester and Delaware County Medical Society, Pennsylvania. No. 1. September, 1853. 32 pp. 8vo. $1 per annum.

REPORT OF BRITISH ASSOCIATION for 1852. *London*, 1853.

THEODOR FISCHER of Cassel, Germany.—The large number of expensively illustrated scientific works published by Mr. T. Fischer, leads us to mention his name in this place. Among the works issued by him are the following:—Dunker and von Meyer's Palæontographica; W. O. Weber's Tertiärflora der niederrheinischen Braunkohlenformation; Göppert's Beiträge zur Tertiärflora Schlesiens; A. F. Römer's Beit. zur geol. Kenntniss des nordw. Harzgebirges; F. v. Hagenow's Bryozoen der Maastrichter Kreidebildung; R. A. Philippi's Conchylien, with 144 pl; ib. Neue Land- und Süsswasserconchylien, with 44 pl.; ib. Neue See-Conchylien, with 100 pl.; Hermannsen's Indices Generum Malacozoorum Primordia; L. Pfeiffer's Symbolæ ad Histor. Heliceorum; Menke u. Pfeiffer's Zeits. f. Malacozoologie; L. Pfeiffer's Blühender Cacteen; ib. Monographia Pneumonopomorum viventium, etc.

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